Vol. 36, No. 2 March, 1929

PSYCHOLOGICAL REVIEW PUBLICATIONS

Psychological Review

EDITED BY

HOWARD C. WARREN, Princeton University
MADISON BENTLEY, Cornell University (*J. of Exper. Psychol.*)
S. W. FERNBERGER, Univ. of Pennsylvania (*Bulletin*)
W. S. HUNTER, Clark University (*Index*), and
RAYMOND DODGE, Yale University (*Monographs*)
HERBERT S. LANGFELD, Princeton University, *Business Editor*

ADVISORY EDITORS

R. P. ANGIER, Yale University; MARY W. CALKINS, Wellesley College; JOSEPH JASTROW, University of Wisconsin; C. H. JUDD, University of Chicago; ADOLF MEYER, Johns Hopkins University; W. B. PILLSBURY, Univ. of Michigan; C. E. SEASHORE, University of Iowa; G. M. STRATTON, Univ. of California; MARGARET F. WASHBURN, Vassar College; JOHN B. WATSON, New York; R. S. WOODWORTH, Columbia University.

CONTENTS

The Psychology of Controversy: Edwin G. Boring, 97.

Horizontal Eye Movements at the Onset of Sleep: Walter R. Miles, 122.

Theoretical Studies from the Harvard Psychological Laboratory: Faculty Psychology: Carroll C. Pratt, 142.

A New Classification of Tonal Qualities: Christian A. Ruckmick, 172.

PUBLISHED BI-MONTHLY
FOR THE AMERICAN PSYCHOLOGICAL ASSOCIATION
BY THE PSYCHOLOGICAL REVIEW COMPANY
PRINCE AND LEMON STS., LANCASTER, PA.
AND PRINCETON, N. J.

Entered as second-class matter July 13, 1897, at the post-office at Lancaster, Pa., under Act of Congress of March 3, 1879

Psychological Review Publications

OF THE

American Psychological Association

EDITED BY

HOWARD C. WARREN, PRINCETON UNIVERSITY (*Review*)
RAYMOND DODGE, YALE UNIVERSITY (*Monographs*)
SAMUEL W. FERNBERGER, UNIV. OF PENNSYLVANIA (*Bulletin*)
W. S. HUNTER, CLARK UNIVERSITY (*Index*)
MADISON BENTLEY, CORNELL UNIVERSITY (*J. of Exp. Psych.*)
HERBERT S. LANGFELD, PRINCETON UNIVERSITY, *Business Editor*

WITH THE COOPERATION OF
MANY DISTINGUISHED PSYCHOLOGISTS

PSYCHOLOGICAL REVIEW

containing original contributions only, appears bimonthly, January, March, May, July, September, and November, the six numbers comprising a volume of about 500 pages.

PSYCHOLOGICAL BULLETIN

containing critical reviews of books and articles, psychological news and notes, university notices, and announcements, appears monthly, the annual volume comprising about 720 pages. Special issues of the BULLETIN consist of general reviews of recent work in some department of psychology.

JOURNAL OF EXPERIMENTAL PSYCHOLOGY

containing original contributions of an experimental character, appears bimonthly, February, April, June, August, October, and December, the six numbers comprising a volume of about 500 pages.

PSYCHOLOGICAL INDEX

Is a compendious bibliography of books, monographs, and articles upon psychological and cognate topics that have appeared during the year. The INDEX is issued annually in June, and may be subscribed for in connection with the periodicals above, or purchased separately.

PSYCHOLOGICAL MONOGRAPHS

consists of longer researches or treatises or collections of laboratory studies which it is important to publish promptly and as units. The price of single numbers varies according to their size. The MONOGRAPHS appear at irregular intervals and are gathered into volumes of about 500 pages.

ANNUAL SUBSCRIPTION RATES

Review: $5.50 (Foreign, $5.75)
Journal: $6.00 (Foreign, $6.25)
Bulletin: $6.00 (Foreign, $6.25)
Monographs: $6.00 per volume (Foreign, $6.30)
Psychological Index: $4.00

Current Numbers: **Review** or **Journal**, $1.00; **Bulletin**, $.60

Review and **Bulletin**, $10.00 (Foreign, $10.50)
Journal and **Bulletin**, $11.00 (Foreign, $11.50)
Review and **Journal**, $10.00 (Foreign, $10.50)
Review, **Bulletin**, and **Journal**, $15.00 (Foreign, $15.75)
Review, **Bulletin**, **Journal**, and **Index**, $18.00 (Foreign, $18.75)

Subscriptions, orders, and business communications may be sent direct to

PSYCHOLOGICAL REVIEW COMPANY,
PRINCETON, N. J.

VOL. 36, No. 2 March, 1929

THE PSYCHOLOGICAL REVIEW

THE PSYCHOLOGY OF CONTROVERSY[1]

BY EDWIN G. BORING

Harvard University

Ideally, it might be argued, the psychologist is a superior being, for over all other scientists he has the advantage of being a psychologist. He alone, the argument would continue, knows the human mind without which there could be no science. The work of the exact sciences, as they are sometimes called, involves not only precise observation but also a loose admixture of personal prejudice, ambition and conviction. The psychologist, however, knows the human mind that is both the object and the subject of his work, and is superior to prejudice, to exaggeration, to vanity, and consequently to quarrelsomeness. Thus without these constant errors he serenely pursues his way, at peace with his fellow-workers, his hand alone grasping at fundamental truth with the personal equation of observation accounted for and eliminated by corrections. Psychology would thus be the one perfect exact science.

And it might be, though it is not clear that it could be. Certainly psychology has not been above personal bias. It is true that when psychologists battle they may hurl Freudian explanations of each other at each other. They may rise with scientific magnanimity against opponents and suggest that falsification is involuntary and unconscious, or that stupidity is inherited and therefore not a matter of individual responsibility. All this is, perhaps, delightfully scientific,

[1] Address of the President before the American Psychological Association at New York, December 28, 1928.

and yet there nevertheless remains in such controversy a seeming lack of objectivity. For instance, some psychologists in writing for publication place the mystic symbol Ph.D. after their names, but none, as far as I know, has yet seen the value of adding the statement of his own I.Q. Stanford University now does that sort of thing posthumously for the great, but it has not yet undertaken a handbook of the living. The classical method of psychology is introspection, yet not the behaviorist, nor the 'gestaltist,' nor the purposivist, nor the late functionalist, nor even the introspectionist himself has yet succeeded in maintaining clear vision with the eye rotated through 180° to see the mind that is at work. From this point of view we would seem to have a long way to go, and yet I must confess to you, attractive as my picture is, that I am not sure that we want to go, or can go, all that way. The scientific eye sees dimly when it turns through half a circle to look behind itself. The scientist, it seems to me, is limited by certain paradoxes of human nature and the psychologist shares these limitations with other scientists. It is therefore to two of these paradoxes that I ask your attention. The second follows from the first.

I. The history of science, like Hegel's view of the history of thought, is one long series of theses, set off by ardently advocated antitheses, with ultimate syntheses terminating controversy and marking a step forward. This picture, it seems to me, holds, not only for speculative, philosophical psychology, but also for the most rigorously observational work. Controversy has always been part of the method of science. A judge, or even a lawyer, might accept the statement that controversy, the clash of prosecution and defense, is the fundamental method for getting at truth. However, I do not think that the scientist would be quite so ready to subscribe whole-heartedly to this principle. He expects controversy as part of the scientific 'game,' but he generally engages in it under the principle that 'I am right and you are wrong.' We hear little in science of an able defense of a lost case. Only correct discoveries are held to measure scientific ability. There is little applause for the investigator

who, by being brilliantly wrong, prevents his antagonist from being wrong at all, and thus contributes to the truth. Unfortunately this situation makes scientists tend to hold to lost causes, when they might know better, for no other reason —unconscious reason—than that the laurel commands no acclaim when shown to be artificial.

After much thought about the matter, I have come reluctantly to the conclusion that scientific truth, like juristic truth, must come about by controversy. Personally this view is abhorrent to me. It seems to mean that scientific truth must transcend the individual, that the best hope of science lies in its greatest minds being often brilliantly and determinedly wrong, but in opposition, with some third, eclectically minded, middle-of-the-road nonentity seizing the prize while the great fight for it, running off with it, and sticking it into a textbook for sophomores written from no point of view and in defense of nothing whatsoever. I hate this view, for it is not dramatic and it is not fair; and yet I believe that it is the verdict of the history of science.

The paradox then in science would seem to be that the more you fight for the truth the less you see it. If you are always trying to see it, you have no time to fight, and without fighting you get science nowhere; you are just the cautious critic who is afraid to venture research. Perhaps I can improve this doleful picture before it is done. Let us, however, turn our attention first to famous controversies, and, since discretion behooves us, chiefly to those of the dead.

If we are looking for controversy, there is of course the case of Mesmer, who participated in a battle royal with the conservative scientists of Paris at the end of the eighteenth century.[1a] Certainly the egotistic, opinionated Mesmer was an uncomfortable person for the scientific contemporary who did not believe in his theory; but, if science is, as is so often claimed, quite impersonal, Mesmer's personality ought to have nothing to do with the question of his demonstration of

[1a] In general on F. A. Mesmer, see Binet, A., and Féré, C., 'Le magnétisme animal,' 1887, Eng. trans. 1888, Chap. I.; Moll, A., 'Hypnotism,' various eds., Chap. I.

the truth of animal magnetism. Nobody knows whether Mesmer in young manhood was more conceited than the average of men who later become famous. It is quite clear, however, that, even had he been a modest and retiring person when young, his great theory that an important beneficent power resided in his own person would have made him into the sort of man that his opponents would regard as vain. In fact, conceit was involved in his scientific theory, since the theory had to do specifically with Mesmer. Yet nobody in the various investigating committees or in the Académie des Sciences was objective enough to see this crucial point. They denounced Mesmer, largely, so it seems to me, because of his personality, and thus mingled their own personalities with their scientific criticism.

The facts can be restated briefly. Mesmer was a physician in Vienna. As a physician he could not ignore the obvious influence of the celestial bodies upon human well-being, a fact well known then although not now. Mesmer asked himself how these remote bodies can act upon human beings from a distance, and it occurred to him that the astrological influence might be mediated by magnetism, a force that is capable of penetrating space. This was Mesmer's first theory, and if he had, then and there, written a book about it we could have put him down with all those persons, from the Greeks to the present day, who have held and advanced theories without resort to experimental proof. Mesmer, however, in good scientific fashion, used his happy thought as the basis for an experiment. He got himself magnets and tried passing them over the bodies of his patients with remarkable effects; the persons were benefited by the magnets or even cured of diseases. 'By the magnets'—it is a proper phrase to have used, and yet so often the supposed analysis of an effect into its causes turns out to be wrong. Presently Mesmer met a Swiss priest who was practicing the same kind of therapeutics as Mesmer, passing his hands over the bodies of his patients *without* the magnets. Mesmer had fixed upon the wrong cause of his effect.

There were two things for Mesmer to do. The more

probable thing—so I am obliged by the history of science to conclude—would have been for Mesmer to have denounced the priest or at least to have tried to prove that he had concealed magnets up his sleeves, to have come out more vigorously in favor of magnets, insisting that they were essential to the true Mesmeric method, and to have enlarged, by the advertising that dogmatic assertion in the face of controversy gives, his medical practice. Mesmer, however, did the improbable thing: he discarded the magnets. It would have been lucky for all the people who have wrangled over this matter for a century after him, if he had discarded the word 'magnetism' also. Unfortunately Mesmer had begun with the notion that a mysterious influence without contact is likely to be magnetic, and the physiologist, van Helmont, a century earlier had expounded a theory of animal magnetism. So Mesmer called his new therapeutic means 'animal magnetism,' allowing the implication to stand that the influence was something like mineral magnetism. He now knew that he could cure people of certain diseases, or make them think themselves cured, which, in certain cases, is the same thing from the physician's point of view. Others could not effect these cures. Undoubtedly Mesmer's personality and his growing confidence in his power were the reasons for his power, but Mesmer did not know this. Not all minerals are magnetic; why should all persons be magnetic? Mesmer came to believe that he, unlike most other persons, was magnetic, and thus capable of influencing others. All this happened before Mesmer left Vienna for Paris in 1778.

Up to this point, it seems to me, we have nothing more than the account of the genesis of a scientific discovery that is without reproach. Mesmer's personality is irrelevant to the scientific fact. The Church opposed him; the scientific academies ignored him; his followers worshipped him; but none of these things matter. He had discovered hypnotism, that is to say, he had discovered the state of hypnosis, had arrived at a vague notion of its therapeutic significance, and was possessed of the practical means of inducing the state, although he had an incorrect theory as to the nature of the

means. Thus Mesmer occupies a definite position in the history of the knowledge of hypnosis. Without him knowledge could not have advanced as it did advance, for it was the travelling mesmerists who interested both Elliotson and Braid, it was Elliotson who interested Esdaile and many others in England, it was Braid who started Liebault on hypnotic work and Liebault who convinced Bernheim who began the Nancy school. Mesmer, ordinarily neglected, occupies the important place at the beginning of the genetic chain of events. That his knowledge of the conditions, the nature, and the effects of hypnosis was incomplete is a situation that applies to almost all scientific discoveries at first; moreover our knowledge of these facts is still incomplete.

After Mesmer went to Paris it is not quite so clear that his discoveries can be divorced from his personality. Here he developed further the conditions of hypnosis, the mysterious baquet with its rods of iron that the patients held (iron, of course, because of its magnetic properties), the circle of sitters about the baquet connected by cords or hands (a circuit, because of magnetic analogy), the subdued light, the soft music, the hocus-pocus of Mesmer's speech and his magician's attire. It is no wonder that the scientific world was disgusted, but my question is whether this disgust interfered with its perception of the truth. Mesmer was the talk of Paris. There was a large band of enthusiastic disciples. The scientists appointed investigating committees, which investigated and found, so it is always said, 'against Mesmer.' Actually there was no denying the phenomena; all the committees did was to disapprove Mesmer's theory which he had formally embodied in twenty-seven propositions, and in particular to deny the identity of this influence with mineral magnetism and the existence of 'a responsive influence between the heavenly bodies, the earth, and animated bodies.' The view then developed that Mesmer, since he was not using mineral magnetism, was employing some secret force that he would not divulge. His disciples, who thought that they had been promised this secret, finally turned against Mesmer because he would not reveal it. Mesmer was discredited,

driven from Paris by public opinion, and died shortly afterward. He had, of course, no secret to reveal. Everybody, the committees and his disciples, knew all that he did, but could not realize that a man can know how to use a power without understanding its nature.

The question of Mesmer's personality comes in here because we wonder whether he merited rebuke. The technique of the baquet was certainly an aid to the technique of hypnotizing, and Mesmer in a sense made this discovery. But was he sincere? Did he believe that all this mystery was an aid to animal magnetism, or did he induce it quite consciously to attract the crowds? The concept of sincerity is a dangerous one. Psychologists could well do without it, substituting the notion of dissociation. At any rate, it seems to me that psychologists who have thought about the problems of personality will have to agree with Mesmer's defenders that Mesmer at least thought he was sincere; and who but a psychologist could undertake to distinguish between a man's sincerity and his belief in his sincerity? However, the conviction or the exoneration of Mesmer hardly matters to us. What about the scientists who repudiated him? They shut their eyes to an important scientific discovery because they could not stomach the conditions of its demonstration. Mesmer was a nuisance. He was a propagandist and a demagogue, and, behold, the whole world had gone after him. Moreover he was making money out of his discovery. He was vain and opinionated, and had even achieved that summit of conceit of making the new force a property of his own person. It is thus no wonder that the scientists repudiated him, and it is also no wonder that the use of hypnosis passed from the hands of scientists to charlatans for nearly half a century. This is the scientific dilemma that I am discussing: does science preserve its purity and thus retard its progress by shutting its eyes to partial truths, and does it thus sometimes cut off its nose to spite its face?

I have dealt with Mesmer at length because I want you to be quite clear as to my problem. I could now go on with other instances from the history of our science keeping you

here until early morning, unless, in spite of being psychologists, you should develop free wills and leave. As it is, my love of determinism is too great to risk such an *experimentum crucis*. I shall be brief with my other cases.

The history of Mesmer was repeated with John Elliotson in the forties of the last century, except for the fact that there can be no doubt of Elliotson's complete sincerity, that is to say, of the complete integration of his personality, with no divided knowledge about what he claimed to be the truth.[2] Elliotson was a physician of exceptional native ability who was a member of the faculty of University College in London in the thirties of the last century. Nowadays we should call him a radical. He was always, to the resentment of his colleagues, advocating some new idea, like the use of the stethoscope, just invented, of which they said, 'It's just the thing for Elliotson to rave about,' or the maintenance of a hospital in connection with a medical school, an idea which, however, he advocated successfully. He made some important contributions to *materia medica*, and did not hesitate to ridicule the fallacies of current medical dogma. He was too ardent to be tactful, and consequently he was disliked by most of his colleagues. In 1837 Elliotson acquired the inheritance of Mesmer by witnessing the demonstration of a travelling mesmerist. Within a few days he was mesmerizing the patients of the new University College Hospital and getting what he regarded as beneficial therapeutic results. He was urged to desist on the ground that he was injuring the reputation of the Medical School, but he refused on the opposite ground that truth is more important than a reputation. Within a year the Council of University College had passed a resolution forbidding 'the practice of mesmerism or animal magnetism within the Hospital,' and Elliotson had resigned from the Hospital and from University College never to enter either again. He kept up his crusade. No medical journal, would print his papers so he founded the *Zoist* as an organ of free speech about new things, especially

[2] In general on John Elliotson, see Bramwell, J. M., 'Hypnotism, Its History, Practice, and Theory,' 1903, 3–30.

mesmerism. He was denounced. Medical men would not associate with him. He lost his practice. Feeling ran into intimate channels and he also lost most of his personal friends. Yet Elliotson kept on. Mesmeric hospitals sprang up all over England. He had a group of supporters, but the group did not include many of the reputable medical practitioners of his day.

How far this controversy penetrated into the emotional lives of its participants is illustrated by the following instance. Like Mesmer, Elliotson saw in mesmerism mostly a therapeutic agent, but it was also obvious that the new state might be used as an anesthetic—in those days just before the discovery of the modern anesthetics. In 1842 Ward, a surgeon, amputated a leg of a patient under mesmeric trance.[3] The patient had been suffering excruciating torture from the least motion of an ulcerated knee-joint, and could sleep little. A mesmerist, Topham, one of Elliotson's disciples, found that he could give this patient rest by mesmeric sleep. Later Ward amputated the leg at the thigh after Topham had mesmerized the patient, and tried, in the course of the operation, bruising the cut end of the sciatic nerve. The patient remained in relaxed sleep and denied all memory of the operation afterward.

Ward then reported the case to the Royal Medical and Chirurgical Society of London. The report aroused a storm of protest. Marshall Hall, whom we now honor for the discovery of reflex action, described mesmerism as 'trumpery which pollutes the temple of science,' and fell back on his own theory, arguing that the report was false because it did not show that the sound leg twitched reflexly when the other leg was cut. Eight years later Hall informed the Society that the patient had confessed to collusion, although the patient then signed a deposition stating that the operation had been painless. Other members at this first meeting of

[3] The full description of the Ward case and of the action of the R. M. C. S. upon it are given in a little pamphlet by John Elliotson, 'Numerous Cases of Surgical Operations without Pain in the Mesmeric States; with Remarks,' 1843. The 'remarks' are numerous and caustic. The pamphlet I have seen was published in Philadelphia, but I think it was also printed in London.

the Society contended that, 'if the account of the man experiencing no agony during the operation were true, the fact was unworthy of their consideration because pain is a wise provision of nature, and patients ought to suffer pain while their surgeon is operating.' At the next meeting of the Society, after violent discussion, it was voted to strike from the minutes the statement that such a paper had been read.[4]

Well? Intolerance does not beget tolerance. That is all. Hypnosis may not be the ideal surgical anesthetic, but it is a great deal better than none, as Esdaile, inspired by Elliotson, was in the same years proving in hundreds of cases in India, and against opposition almost as strong as was to be found in England.[5] The medical men almost let the world suffer on in surgical operations for an indefinite period. They might have done so but for the fortunate discovery of the anesthetic effects of nitrous oxide in 1845, three years after Ward's use of mesmerism, and of ether, and chloroform a couple of years later.[6] Against these anesthetics there is a story of similar opposition and of the contention that anesthesia interferes with God's plan for the universe; but I have cited enough instances of this sort.

In fact the history of science is full of such examples. Elliotson made them his text when, after much opposition, he was finally invited in 1846 to deliver the Harveian Oration before the Royal College of Physicians. He could begin most aptly with the story of the opposition to Harvey's discovery of the circulation of the blood.[7]

In modern psychology we have so far been spared the

[4] I have omitted all mention of James Braid, the reputed discoverer of hypnosis, on account of lack of time. See Braid, J., 'Neurypnology,' 1843, reprinted 1899. Braid also met opposition, but he did not break with the medical profession because he refrained from criticizing it, because he laid no claim to a peculiar personal power but sought to explain hypnosis in normal physiological terms, because he avoided the word 'mesmerism,' because he opposed Elliotson, and because Elliotson attacked him. See my remarks on this situation in *Amer. J. Psychol.*, 1927, 39, 83–86.

[5] The secondary source for James Esdaile is Bramwell, *loc. cit.*

[6] For an interesting account of the controversy that the discovery of anesthesia aroused, see Smith, C. A. H., *Scient. Mo.*, 1927, 24, 64–70.

[7] Elliotson, 'The Harveian Oration,' 1846,

violent controversy that engages public attention, except in psychic research, which represents today a case almost exactly like that of mesmerism. It seems impossible to undertake psychic research without emotion, and the emotions of the investigators are present in part because it is an egotistic hypothesis. Like mesmerism it claims that a peculiar power is localized only in certain individuals, and it defines this power in terms of its effects and omits the causal term that is necessary to every scientific correlation or fact.

However, although modern psychology lacks these dramatic controversies that enlist the lay public on one side or the other, it is lacking neither in controversy nor in intolerance. I trust that I am still treading safe ground if I ask you to recall with me the famous controversy between Wundt and Stumpf about the tonal distances.

Into the elaborate intricacies of this controversy we cannot enter, nor do we need to do so. As is well known, musical interval follows a law like Weber's Law. A given interval is divided into two equal portions by a stimulus which is, in vibration rate, the geometric mean of the stimuli for the two extremes. Stumpf, with a musical background, believed that musical interval bore a close relation to the simple sensory properties of tones. Wundt, basing his view on experiments in the Leipzig laboratory by his pupil Lorenz, regarded sense-distances as less closely related to musical interval. Lorenz' results showed that observers in bisecting a tonal interval tended toward the arithmetical mean and not the geometric. About this difference the controversy waxed.

We should perhaps bear in mind the fact that the difference in question is small with respect to the tonal distances involved. On the other hand—and this was Wundt's ground for assurance—these seemingly small differences were large with respect to the scatter of the judgments, much larger than modern statisticians are accustomed to require. Stumpf, however, could not accept this view. For one thing he appealed to the extreme case as a *reductio ad absurdum;* if tonal distance is directly proportional to vibration rate, as

Wundt claimed, then a major second like $c^3 - d^3$, must include the same distance as the entire octave, $c - c^1$, three octaves below. This proposition seems so manifestly absurd that we can understand why Stumpf felt that Lorenz's results must be capable of being explained away, as he undertook to do argumentatively, in part by questioning the meaning of Lorenz's observers in judging tonal distance and the degree to which they were influenced by musical relationships.

Wundt had espoused Lorenz's results by publishing some of them in the third edition of the 'Physiologische Psychologie' in 1887.[8] Lorenz's paper came out in 1890.[9] Then followed controversy, altogether of 141 pages. Each published thrice. First Stumpf printed sixty-seven pages, in which he reprinted portions of many of Lorenz's tables and sought to reinterpret them.[10] There was almost no personal invective in the paper; nevertheless it is hardly impersonal to reprint another man's results and, in a paper almost as long as the original, argue elaborately to opposite conclusions. It is easy to imagine Wundt's feelings when the significance of the observations was thus called in question. Wundt, therefore, replied with a paper which included some personal advice to Stumpf.[11] Stumpf's rejoinder adopted more nearly Wundt's tone.[12] It was called 'Wundt's Antikritik.' Then Wundt printed 'Eine Replik C. Stumpf's.'[13] Finally the controversy closed in verbal exhaustion with Stumpf's 'Mein Schlusswort gegen Wundt'[14] and Wundt's 'Auch ein Schlusswort.'[15] The discussion became less calm as it progressed. The two final *Schlusswörter* dealt each almost as much with the psychological problem of how the other psychologist conducted argument as with the psychological problem of tonal distances.

[8] Wundt, W., 'Physiologische Psychologie,' 1887, I., 428*f*.
[9] Lorenz, C., *Philos. Stud.*, 1890, 6, 26–103.
[10] Stumpf, C., *Zsch. f. Psychol.*, 1890, 1, 419–485.
[11] *Philos. Stud.*, 1891, 6, 605–640.
[12] *Zsch. f. Psychol.*, 1891, 2, 266–293.
[13] *Philos. Stud.*, 1891, 7, 298–327.
[14] *Zsch. f. Psychol.*, 1891, 2, 438–443.
[15] *Philos. Stud.*, 1892, 7, 633–636.

This controversy must be read to be appreciated, but I can perhaps give you the flavor of it. As I have said, Stumpf in his original criticism had little to say to which Wundt or Lorenz could have objected, except that they were wrong and should have drawn exactly the opposite conclusions. The only definite resort to the method of psychologizing opponent psychologists that I have found in the entire paper is this: "This extension, however, certainly does not amount to as much as it should according to Wundt, who here, as he so often does, has exaggerated a correct idea into another that is falsely inverted with respect to it."

Wundt in his reply studied to be calm. He said twice that he would test Stumpf's conclusions 'sine ira et studio,' without anger and vehemence. Such a statement, however, carries a latent as well as a manifest content. When the orator says, 'I will call my opponent neither a liar nor a fool,' he is not doing just exactly what he says he is doing. There is not the least doubt at all that Stumpf had hurt Wundt's feelings, and that Wundt was thus moved to many of his remarks, including his final sentences, which read as follows: "Stumpf knows, I hope, as well as I, that whoever would further the psychology of tone must have something more than musical experience. However, it can do no harm, I believe, if he will strengthen himself in this conviction by the result which he now achieves [as the consequence of this criticism]. Somewhat sooner then will this polemic also have for him the further result, that he will learn to value, not only as the best but also as the most useful virtue for a scientific researcher, this: to be just toward others, to be severe toward himself." This peroration does not seem to me to clinch the problem of the tonal distances. I suppose, however, that Wundt thought it did.

I have not time to cull numerous examples from the remaining four articles. You can imagine what was said after Wundt has thus advised Stumpf 'without anger and vehemence.' To Wundt's personal advice about being just toward others and severe toward himself, Stumpf, everything considered, replied quite calmly. He said, "Wundt is ac-

customed to imprint on his polemics a kind of moral stamp. . . . It is distasteful to me to make many words about the matter." Wundt reiterated that he found nothing in Stumpf's rejoinder "from beginning to end but distortion and fictions. 'I have studied these things and you have not!' Upon these words I restrain myself from judgment," he concluded.

Finally Stumpf, who had been consistently the more reluctant to pursue the personal side of this controversy, was goaded in his *Schlusswort* into a frank characterization of Wundt's polemical method. He wrote: "I abstain from a detailed rejoinder to the new voluminous reply of Wundt. For it, which pours out his expression of blind thoughts, any word would be too much. Those, however, who wish to compare, point for point, his new article with mine and especially with the earlier one upon which it is based, will find therein for themselves, as in his preceding article, the same mixture of untrue assertions, of confusions, of mutilations of the course of my thought, of obscure imputations and negligences, of infirm evasions, of fallacies of every kind, and of frequent assurances of the incapacity and ignorance of his adversary." Each of the first six items of the list Stumpf supported with long footnotes, omitting only citations of his own alleged incapacity and ignorance.

Stumpf started the controversy, but Wundt made it personal. It is plain that Stumpf was drawn into this aspect of it with reluctance, and that, being more tender-minded than Wundt, he felt it keenly. More than thirty years later, in writing a short account of his life and thought, the affair still rankled. He devoted to the controversy a paragraph, which he placed in the biographical half of his article.[16] Stumpf regarded the controversy as an event in his life more than as a psychological contribution; but Wundt, the tough-minded, made no reference to this little affair in his 'Erlebtes und Erkanntes.'

Now I feel that most of you will be disposed to condemn this controversy and to blame Wundt the more for the part

[16] Schmidt, R., 'Philosophie der Gegenwart in Selbstdarstellungen,' V., 1924, 218.

he took, and yet I believe that there are not so many of us who, on the next occasion when our work is attacked in print, will in reply studiously avoid trying to make our antagonist seem to our readers like the fool that we believe him to be. We have not yet solved my fundamental dilemma; we have only illustrated it.

However, before I discuss the major issue, let me point out that controversy of this kind is not limited to Germany. In the nineties there was the American controversy about reaction times, with Titchener and Baldwin the chief protagonists.[17] Titchener was upholding the Leipzig view that the muscular reaction is always about one tenth of a second shorter than the sensorial, provided you have subjects so well practiced that they can assume the two attitudes at will. Baldwin was contending that people are of different types and that some react more quickly in a sensory manner and some in a motor manner. Baldwin thought that Titchener was misrepresenting the truth by selecting subjects that would fit his theory. Titchener thought that Baldwin had wandered from the straight scientific path in concerning himself with a problem of human nature instead of the scientific problem of the generalized human mind. Of course, as Angell and Moore showed eventually, both were right; yet neither seemed to be able to see how the other was right, obvious as the matter is now. If Baldwin wanted to work with individual differences in true American fashion, what matter if Titchener thought that personal idiosyncrasies are not the problem of science. If Titchener got his difference with *general* practice (not, of course, with practice for giving the desired result), why did Baldwin mind that training in the direction of attention should counteract the effect of natural modes of attention? Yet each was so sure of his view that neither ever seemed in publication to understand the other. Each, like Wundt and Stumpf, made a moral judgment. Titchener thought Baldwin unscientific because he used subjects untrained, in the Leipzig sense, to precise

[17] The chief controversial papers are: Baldwin, J. M., PSYCHOL. REV., 1895, 2, 259–273; Titchener, E. B., 'Mind,' 1895, N.S. 4, 74–81, 506–514; Baldwin, *ibid.*, 1896, N.S. 5, 81–89; Titchener, *ibid.*, 236–241.

observation. Baldwin thought Titchener unscientific because he closed his eyes to a problem of the natural world.

Nor is controversy of this sort limited to the ancients of the late nineteenth century. I could have taken my examples from the present decade, but I have thus far forborne because it must be hard for you to believe that my remarks carry with them no whit of praise or blame. Perhaps I can briefly make my point that the styles have not greatly changed by a few citations in which I obscure the source.

Only a few years ago one psychologist complained, in a long critical article, about the practice of a colleague, who, he asserted, would praise the work of his friends and condemn similar work by others. This controversy is full of instances germane to my subject-matter, but I shall content myself by citing only the closing sentence of the paper I have mentioned. It reads: "We live close to one another with our similar problems, which approach today as nearly as does all the community of work. If here, as in a thickly planted forest, conflicting growth occurs, it is a thing of the natural order. If, however, it falls out as in a horse-race, where someone uses the whip in order to lash the noses of the neighboring horses, then I must raise a protest against it in the name of fair play." [18] Recently a psychologist, usually very conservative in his utterances, actually likened a colleague to 'a soap-box evangelist.' Within the year another psychologist has said, in print, of still another: "To the charge of misunderstanding must now be added the charges of misreading, misinterpreting, and misquoting," and then, like Stumpf in his *Schlusswort* to Wundt, has proceeded in two pages to document these items. It sounds scandalous, that a scientist should not only misread and misinterpret, but actually misquote. Yet I doubt if either author is less well-intentioned than the other.

You may say, of course, that all this is but the scientific 'game,' that it is the way things are done. I submit, however, that these expressions are not mere stylistic conventions of writing; often there is even more real feeling than the words

[18] The quotation is as literal as anonymity permits.

express. Wundt's moral prescriptions for Stumpf in 1891 were still disagreeing with him in 1924. We have all known psychologists who were supposed not to be able to meet each other socially lest something should happen. Most of us know what it is to feel bitter about published criticism, especially when it is personal; yet, if science is the dispassionate search for truth by the empirical method, can it flourish in the face of passion?

Let us go back to Wundt and Stumpf. The argument is, so far as it dealt with the tonal distances, very evenly balanced. Titchener said that he read the controversy three times, and decided twice for Wundt and once for Stumpf.[19] Since we still do not know the correct answer to their problem, we might say that the chances are even for either of them being right; and thus the chances are even for either being wrong. It was a battle of giants; why discriminate? But, if scientists are seeking only for the truth and not to prove themselves right, then there are even chances that Stumpf would have convinced Wundt, or Wundt Stumpf, and only a twenty-five per cent chance that each would have convinced the other and have thus continued the controversy. I make this ludicrous use of the elementary principles of probability in order to show you how certain you would have been from the start that neither was going to be convinced. They both could not be right; each knew that; each, as a psychologist, knew about human fallibility and prejudice even in the pre-Freudian days, and could therefore realize that there was a good chance of his being somewhere in the wrong. Plainly there is a perseverative tendency in scientific thinking.

It would be easy now to draw the moral that the scientific value of an investigator varies inversely with his emotionality in scientific matters, but I do not believe that such a conclusion would be true. It is not only the lesser men who quarrel. The great are particularly adept at it, and the lesser may perhaps only be copying them. Rather it seems to me that we have a true dilemma, that the drive that urges men to laborious research and to the braving of public

[19] Titchener, 'Experimental Psychology,' II., ii, 1905, 242.

criticism with their conclusions, is the drive which perseveres and makes them persist against criticism. Thus the same thing that drives them toward the truth may also keep them from it. We still face, then, the uncomfortable picture with which I began. However, before I attempt even an incomplete solution, I want to deal briefly with the second of my scientific paradoxes, which I promised you long ago.

II. This second paradox is that new movements in psychology, and presumably in thought at large, are most obviously negative. That which claims to be progress, that which is presently accepted as progress, is nevertheless most patently an undoing of the progress of the past. How then is there any real progress in what appears on inspection to be a regress? The answer, I think, is psychological, but before I come to it, let me try to establish my point about the negativism of progress.

Recently I have tried to show for psychology that trite historian's point that nothing which is supposed to be new is ever really new.[20] The course of scientific thought is gradual, as it is in individual thought. In the individual it is hard to distinguish imagination from memory; careful scrutiny of a creative imagination seems to reveal little that is brand-new. So it is in scientific thought. The ideas occur as the result of individual thinking, or the facts are found as the result of experiment, both are put forward, and nothing much happens. Then, perhaps many years later, someone comes along, sees relationships, puts things together, and formulates a great theory or founds a great movement. Often the formulator or founder is not even the compounder, but another man, who because of his personality or because of the times in which he speaks, has the capacity for gaining attention. So he originates, as we say, a step in progress, lending his name to a theory or a school, and it is left to dull historians to discover and reiterate the fact that de Moivre discovered the Gaussian law and Charles Bell the Müllerian doctrine of the specific energy of nerves. Founders are generally promoters, in science as elsewhere, and we have

[20] *Amer. J. Psychol.*, 1927, 39, 70–90.

therefore here to consider the mechanisms of public attention.

With respect to scientific movements there seems to exist something like Newton's third law of motion: action equals reaction. You cannot move—in the sense of starting a movement—unless you have something to push against. The explanation of this law, I think, lies in the relation of movements to public attention. Science can actually, by the empirical method, so I am disposed to believe, lift itself by its own boot straps, but the result is not what we call a 'movement' because motion can be defined only with respect to a frame of reference. A movement must move with respect to something, and progress must move away from something, if the movement is to command observational attention. It is therefore the business of the founders of new schools, the promoters and propagandists, to call persistent attention to what they are not, just as one political party is forever emphasizing the short-comings of the other.

Thus we see that movements are founded upon controversy, and that all we have been saying about the effect of controversy on controversialists applies also to the schools. A school may be flexible and disposed toward change and growth in all directions except those against which it has set itself. Here it is hardened by its own drive. A movement cannot move backwards and persist, and the question as to which direction is backwards is decided by the opposition which brought the movement into being. Moreover the drive forward leads to an over-estimation of the distance moved. The negativism of progress is thus essential to observed progress.

Now let me illustrate.

The greatest foundation within modern psychology is Wundt's promotion of experimental psychology itself. The question is often asked: Did Fechner or Wundt found experimental psychology? Fechner came first and may be its father, but Wundt is certainly its founder. Fechner with his psychophysics was trying to found, not experimental psychology, but a spiritualistic metaphysics. Wundt, from within physiology, arrived at his view from a study of the

relationships of the sciences, in a day when physiology was as self-conscious as psychology is now.

In the interests of the new movement Wundt had to overcome many obstacles. He had to write a scientific handbook for experimental psychology. He had to get himself a chair of philosophy and pervert it to experimental practices. He had to found a laboratory, a real laboratory of rooms with instruments in them. He had to get the experiments going, and then to found a journal for their publication. To make his point it was necessary for him, in all sincerity, to exaggerate. The new experimental science must be exhibited to the world as a lusty infant with none of its organs missing. Thus Wundt, when experimental results were lacking, resorted in his handbook to speculation to fill the chapters. There was certainly an over-emphasis on apparatus, peculiarly psychological apparatus. If psychology was an independent science, it must have apparatus to distinguish it from philosophy, and special apparatus to distinguish it from physiology.

All this we can readily understand because we ourselves are still of this self-conscious school of Wundt's. The struggle to separate psychology from philosophy in American universities is still not quite yet over. The habit of writing complete text-books in the face of incomplete knowledge still persists. There is still, I believe, a tendency to collect and exhibit much psychological apparatus without regard to the immediate needs of research. If you do not know what it is like to be on the inside of a new movement, consult therefore your own minds.

Yet this movement for a scientific psychology was largely negativistic. It was primarily directed against philosophy. It was a long time before Wundt had done any experimental work equal in importance to Fechner's, and yet Fechner thought he was working in experimental philosophy. The experimental work of the sixties and seventies was performed mostly by physiologists. Of course, we say now that the final result has demonstrated the positive nature of the original idea, although there remain philosophers who do not

agree. I do not believe, however, that the present outcome reacts upon the situation of sixty years ago. Whatever has happened since, there was a chance then that experimental psychology might prove sterile. But it is difficult to argue clearly where our own prejudices are involved. Let us consider the movements within psychology.

In the nineties there was the school of *Gestaltqualität.* It was a reaction against the current elementarism, although it did not itself avoid elementarism as successfully as does the modern *Gestalt.* The chemical combination of sensations was obviously inadequate for the explanation of perceptions. Nevertheless the form-qualities, the founded contents, the superiora, and the act of founding turned out to have no empirical definition and the movement failed. Or did it not fail, but live on to be reborn in *Gestaltpsychologie?* The answer does not matter. My point is that it would not have been a movement if it had not been directed against something.

So it was also with Külpe's school of imageless thought. The very word 'imageless' is a negative term. The movement was nothing more than a protest against sensationism. It is easy to say this now, but what of the enthusiasm of the Würzburgers for the *Bewusstseinslagen* and *Bewusstheiten?* They did not think that their movement was negative. They thought they had discovered a new kind of mental stuff. You have only to read the controversial literature to see how the love of self-preservation sustained each side.

In America we used to have functionalism. It was a revolt of the colonial psychologists against Germany, their mother-country. The controversy between Titchener and Baldwin was a phase of the whole. Germany was more philosophical and America more practical, as America's rôle in the history of mental tests has shown. Chicago functionalism was the explicit movement, but I think it was but symptomatic of what was quietly going on all over America, except in some protected places like Ithaca, where Penelope still remained faithful to the marriage vow. Functionalism centered attention upon the individual and the individual

organism. Leipzig could still work with the generalized human mind; in Chicago, and in Columbia too, they had *minds*. I think of this revolt as the most radical since Wundt's original heterodoxy, and I also recall that the explicit functional movement itself was largely negative and got little further along positively than did the school at Würzburg.

In those days the opposite of functionalism was structuralism, but nobody—except perhaps some graduate students—ever called himself a 'structuralist.' Titchener adopted the phrase 'structural psychology' and abandoned it long before it went out of use. No, the functionalists had to have something definite to push against, and it was they only who talked about 'structuralists.'

We have this same phenomenon in behaviorism. For years the American tendency has been to have two behaviorists growing where one grew before. Any number of psychologists have been willing to call themselves behaviorists and to be proud of it, but they missed badly a definite opposition to set them off. Words have been coined for the opponent school, words like 'introspectionism' or 'introspectionalism,' but I have never heard anyone apply such a term to himself. Someone once suggested 'Titchenerism,' which had the advantage of seeming to indicate at least one Titchenerist definitely. My point is that all along behaviorism has been seeking an enemy so that it could disprove the charge that it is fighting windmills, for it must fight something; it is a movement.

I know it is not fair to leave behaviorism so casually, but I must do so. Behaviorism is not new; this has been shown more than once. Yet Watson is right in thinking that he founded it. He could not have founded it if it had been new; it would not yet have been ready to found. It denies consciousness as the subject-matter of investigation, and therefore the so-called introspective method for investigating it. In this it is negative. It goes on investigating what is left, bereft of an enemy since many of those whom it woos for enemies would also investigate the same problems. It is

unfortunately limited by its parental inheritance, for it cannot get over trying to translate consciousness and the sensory quale into behavioristic terms, as it has already translated association into the conditioned reflex. Respect for parents may be laudable and yet hinder the free development of youth. Behaviorism is already past its prime as a movement, because movements exist upon protest and it no longer needs to protest. Had it been less successful it might have lived longer as a movement and a shorter time as a method.

Gestaltpsychologie is in the same box with behaviorism. Born at the same time its development was hindered by the war, so that it is now less mature. Its infant cries of protest against an unkind world still persist. Everybody must know now what *Gestaltpsychologie* is not. It is not elementaristic or associationistic. It eschews the vague concepts of the past, like attention and attitude, and cultivates new vague concepts in their place, like insight, closure, and level. When Wertheimer and Koffka were describing it, they worked largely in negative terms. There is no general positive content of *Gestaltpsychologie* with which anybody disagrees. Still the voice cries in the wilderness, whereas the kingdom of God is already with man. *Gestaltpsychologie* was not new in 1912; it was quite ready to be founded. It is now a movement. Presently, I think, it too will become simply psychology.

I am now ready to form a conclusion.

I believe that I have shown that movements and the rise of schools are a form of controversy, often one-sided because directed against no particular antagonist. Thus, as controversy, the movement introduces all the psychological advantages and disadvantages of personal controversy.

Discussion is relevant to scientific work, but controversy is more than discussion. It involves emotion; and passion, while of itself irrelevant to scientific procedure, enters to prejudice reason and to fix the debaters more firmly in their opinions. If it were possible, scientific discussion should be dispassionate, not only in form but in spirit, for otherwise progress toward the truth is hindered.

Since the controversy of a movement is apt to be less personally pointed, especially when there is only one active party to the quarrel, participation in a movement may have the advantage of blinding the scientist less than participation in a personal controversy. On the other hand, movements, in so far as they are blind, have the further disadvantage of lending to blindness the social support of the group within the school.

As psychologists, we cannot, however, afford to condemn controversy, be it ever so emotional. If we could read out of the body scientific every investigator who lost his temper with an opponent and kept it lost, we should read out those very men who, because of their drives or prejudices or whatever we like to call that conative component of their personalities, had made the positive contributions to the science. Research is something more than a habit and it requires something more than patience. It requires, among other things, an irresistible urge, bolstered up, I think, not so much by curiosity, as by egotism. This urge may carry one to the truth, beyond it, or even directly away from it. Vision and blindness are here alike, for both are attention, and attention to one thing is inattention to another. The same urge helps and hinders progress.

Must the truth then forever transcend the individual? Is the stage of science like the court of law, where attorneys contend and only the judge speaks the truth? This is the view of research that I find so personally abhorrent and yet seem forced to accept.

There is, however, an incomplete solution for the dilemma. A scientist should, I think, cultivate dissociation. Too much has been said in favor of the integration of the personality, and too little in favor of dissociation. The scientist needs to be a dual personality. He needs to be able to become the prosecutor or the judge at will. He can then stand off and evaluate himself at times, and perhaps even arrange things so that the prosecuting personality will fare more happily when it returns to dominate his person. But I would not have him be the judge too often, for then the assured,

prejudiced, productive personality might get 'squeezed out,' and science would be the loser.

I recommend this dissociation, not because it will make us happier, not merely because it is fun to be the judge as well as the prosecutor, but because I have no expectation that it could be so complete that there would be no interaction between the two personalities. I should hope for a tempering of the prosecutor by the judge so that there would really be more vision and less blindness, and so that psychology would benefit thereby. Then we should have less futile controversy, fewer people devoting their lives to lost causes, even more candid and thus more fruitful discussion, less talk and more research.

I have asked you to-night to play the judge with me. I think it is important for psychology, still so talkative a science, that we should all be practiced in being judge as well as prosecutor. Do I dare in closing to point you a moral, as Wundt so ungraciously did to Stumpf? If there is any precept that comes out of all this talk, it is rather that we should beware of precepts. Psychology needs both judiciousness and effective prejudices; and I cannot resist the impression that we shall do well to cultivate and welcome both.

[MS. received January 2, 1929]

HORIZONTAL EYE MOVEMENTS AT THE ONSET OF SLEEP[1]

BY WALTER R. MILES

Stanford University

"His eyen stepe, and rollinge in his heed,
That stemed as a forneys of a leed."
—*Chaucer.*

I

The sparkling and 'rolling' of the eyes and their glowing like a furnace of lead is a description of the most alert wakeful state. The characterization would be in quite other terms if it were intended to portray fatigue or profound drowsiness. The contrast between alertness and drowsiness is nowhere more evident than in the condition and behavior of the eye and its associated tissues. It would seem little short of ridiculous for an actor to simulate normal sleep and not at the same time close his eyes. Children in their play make closing of the eyes equivalent to sleeping; they do not add the changes in breathing and other symptoms.

By common consent sleep has been thought to begin with closure of the eyelids. Thus Posey and Spiller (1) write of the 'sleep-ptosis' as a temporary nuclear paralysis of the levator muscle. It is certainly true that if one tries to induce sleep in himself he closes his eyes. Probably this is not so much to relieve the levator as to lull the retina, for we may judge that darkness and sleep have long been in partnership. Therefore on retiring the eyes are closed, in some cases for several minutes before sleep actually begins. On the other hand if the circumstances are such that the individual strongly desires to remain awake but finds himself momentarily overcome, it is a question whether the sleep

[1] The records illustrated in this paper were taken at the Nutrition Laboratory, Boston, of the Carnegie Institution of Washington prior to 1922. The author expresses his appreciation to Dr. Francis G. Benedict, Director of that Laboratory, for the opportunity to make this use of the records.

precedes the so-called 'sleep-ptosis.' Can a man sleep with his eyes open?[2] A quotation from the remarkable observations of Sir Charles Bell is pertinent: "If a person be fainting, as insensibility comes over him his eyes cease to have speculation; that is, they want direction, and are vacant, and presently the white part of the eye is disclosed by the revolving of the eyeball upwards. So it is on the approach of death; for, although the eyelids be open, the pupils are in part hid, being turned up with a seeming agony, which however is the mark of increasing insensibility" (**2**). Spencer and Gillen (**3**) in their illuminating portrayal of the Australian Arunta record how these natives stand surrounding a dying relative. '*When his eye goes*' an elder brother immediately places the now dead man's body in the burial posture. Similar to the condition noted in fainting and at the time of death in which vision ceases prior to or without lid closure, is that which may occur briefly when an individual is making a strenuous but not quite successful effort to keep awake. We know that at times the demand for sleep is imperious: Damiens the disturber of Louis XV slept upon the rack during the intervals of respite from his torture.

It is a question whether when an individual is asleep with his eyes open an observer can recognize the condition as that of sleep by contrast to the vacant look of marked absentmindedness. Probably the distinction could be made, but I am not aware that anyone has undertaken such a study

[2] In a well-known passage, Pliny, 'Natural History,' Bohn, 1892, 3, 52 states that rabbits and some human beings sleep with their eyes open. He mentions that the Greeks had a word name for this condition.

Professor H. C. Warren in a personal communication gives me the following interesting experience: "When I was a child my left eyelid closed with difficulty on account of a facial burn. I recall distinctly at the age of about seven, being asleep and dreaming of a room with a certain pattern of wall paper; but there was a curious patch of another pattern at one place in the room. This patch moved about. Then I awoke and found that my left eye was open; it was broad daylight and I had been actually seeing the wall paper of the room and fitting it in with the content of the dream."

The condition known as coma vigil in which the person lies with eyes open and can only be aroused with great difficulty from his semi-conscious state, has been judged as distinct from normal sleep, in which the eyelids may be parted but the pupils are ordinarily not visible.

of the first moments of sleep. This illusive interval, when the individual ceases to be awake and begins to be asleep, we will consider in the light of a series of records of the eye movements as they actually occur.

II

There is no direct reading vision-thermometer or vision-plethysmograph which shows whether the individual at a certain moment is truly conscious of the visual experience that we might expect him to have in virtue of the direction of his visual axes. The eyes may be so directed that light reflected or emanating from a certain object falls upon the macula lutea but even so we have finally to depend on the individual himself to tell us if he sees. However, aside from language report, we do have at our disposal, for judging the presence or absence of real vision, the movement behavior of the eyes as they shift to one point and another and exhibit fixation. We are much more expert in interpreting and sensitive to the presence of such behavior than we ordinarily realize. Enough is also known in a scientific way about the characteristics of these movements, speed of shift, duration of clearing up period, corrective adjustment, fixation time, etc., so that some confidence can be placed in an interpretation of an objective eye movement record.

Dodge has taught us how to record the movement behavior of the eyes with the least annoyance to the person observed. The bright spot, image of an arc light properly screened, reflected from the cornea shifts its position in the direction in which the eye moves. This moving highlight can be easily observed and photographed with an ordinary enlarging camera equipped with some kind of a film moving unit.[3] If a timing wheel is used to intercept the path of the light the resulting record becomes a chronophotographic account, in one axis, of the eyes' succession of movements and pauses.

In his investigation on the effects of alcohol Dodge (4)

[3] For a description of simple equipment along the lines worked out by Professor Dodge, see Miles and Shen, J. Exper. Psychol., 1925, 8, 344; for an adequate film moving device which is readily obtainable, see Miles, W. R., *J. Opt. Soc. Amer.*, 1928, 16, No. 4.

used an eye movement test as a part of his battery of neuro-muscular measurements. With the left eye covered the subject fixated alternately two dots placed symmetrically on either side of the primary line and separated by a distance equal to 40° on his arc of vision. Arrangements were made for holding the head still so that the movements would be confined to the eyes. The subject was urged to look precisely at each fixation point each time he came to it. In some instances it was so provided that the fixation points were small letters which could be changed. This caused better motivation for sharp vision. Dodge's eye movement measure has proven to be a singularly important one, due I think primarily to the fact that the extra-ocular muscles seem lacking in sensory receptors or fibres. Normally an individual cannot control the rate of movement of these muscles. The initiation of a movement for shifting the eye from one fixation point to another over the 40° arc seems truly enough a voluntary affair. You can hold your eye on one point or you can glance over to the other, whichever you wish. But if you decide to shift to the second point, the rate of the shift, that is, the speed of the movement that occurs, does not seem to be under voluntary control. A movement of this kind, which we call a saccadic movement is used to shift the line of regard from object to object as when one examines a picture, reads a book, or looks about before proceeding over a railroad crossing. The speed of such a movement is normally quite fast. In reading Chinese it was found by Shen (**5**) that the shift from one reading fixation to another required about 10″, to change from the end of one line to the beginning of the next took about 50″, while the average duration of a reading pause was approximately 300″. Moving the eye 40° alternately between two given points normally requires 100″. Dodge (**6**) has given a short table of speed values for horizontal movements of different amounts. The saccadic time is seen to be too brief to include a conscious reaction interval. After the eye is in motion the subject cannot be aware of the motion early enough to do anything toward directing it voluntarily until after it has come to an end. Saccadic

movements are in this respect like wink reflexes or tendon reflexes. Furthermore, it seems impossible, preliminary to the saccadic to tense the recti so as to bring them into more swift operation. Quite independently of the subject's desires or efforts the abductive saccadics are usually found to be more rapid than the associated adductive movements. It is not our intention to take up at this time the various features of saccadic speed for full analysis. The point of importance for the present discussion is that while voluntary, so far as original initiation is concerned, the saccadic shifts of the eyes are withdrawn from such control in respect to their actual duration. This being the case we may expect saccadic speed to reflect changes in neural condition. Such results have been conspicuous in experiments with alcohol. Dodge and Benedict (4) found that the eye-movements gave a result which was similar to the curve for the total group of measures and they state: "In every respect we believe that the eye-movements are the most reliable and the most important measurements of the group," p. 265. Miles (7) in work with alcohol and also in an extensive study of the effects of low diet (8) came substantially to the same appraisal of eye-movements as an index of subjective condition.

Recently Dodge (9) has published a mass of data on his own reactions collected over a long period. This study is on the influence of different conditions in producing variability in performance. In his Fig. 19, p. 76, it is shown that the speed for 40° adductive saccadics recorded from the right eye with the left covered, varied from 90° to 140+°, with a distinct mode at about 100° to 110°. He shows that the saccadic speed gave a regular curve of diurnal change. His fastest movements were at about 8 A.M. and the slowest recorded were at 2 A.M. He says, "The pronounced depression of the lid-reflexes and eye-movements and their gradually increasing depression throughout the day is undoubtedly conditioned in part by specific fatigue. . . . The curve of their depression throws an interesting light on the commonly observed localization and main indication of sleepiness in the eyes and eyelids," p. 94. In a series of

experiments (Miles (7), pp. 111*ff*), where the subject was measured in the evening, slept all night in a respiration chamber, and was measured again in the morning, it was found that invariably the eye-movements were more rapid in the morning. This slowing up of the saccadics at night can be noted in direct experience. In the morning watching my eyes in a mirror I can see no trace of the movement when shifting the line of regard from the reflected image of one eye to the other, at night when sleepy, I can see some trace of this shift.

It seems justifiable to conclude that the saccadic speed is changed by various conditions of the central nervous system induced by drugs, fatigue and the like. On the other hand the eye movement speed is practically withdrawn from voluntary interference, although it is a coördination controlled from cortical centers.

III

What eye movement record criteria may we use to judge that an individual is awake and looking sharply at fixation points that have been specified? Photographic evidence answers this question. Some sample records (Plate I.) are given as illustrative and our discussion will refer to them. Illustration D is a double record, that is, one series of alternate fixations (40° movements) was recorded, then as a simple matter of space economy the photographic plate was raised, the camera moved slightly to one side and the plate released for recording a second series of fixations.

Consider first the record that occupies the left hand portion of picture D. Beginning at the bottom, the subject looked first at the right mark. The vertical line made by his fixation shows a little wavering, which in this case is due to jostling the camera in starting the fall of the plate. As the subject starts the movement of his eye to the left mark the record turns sharply. The line of dots represents the path and speed of movement. The dots become regularly smaller as the eye approaches the left mark and finally the individuality of the dots is lost as the eye comes to rest and

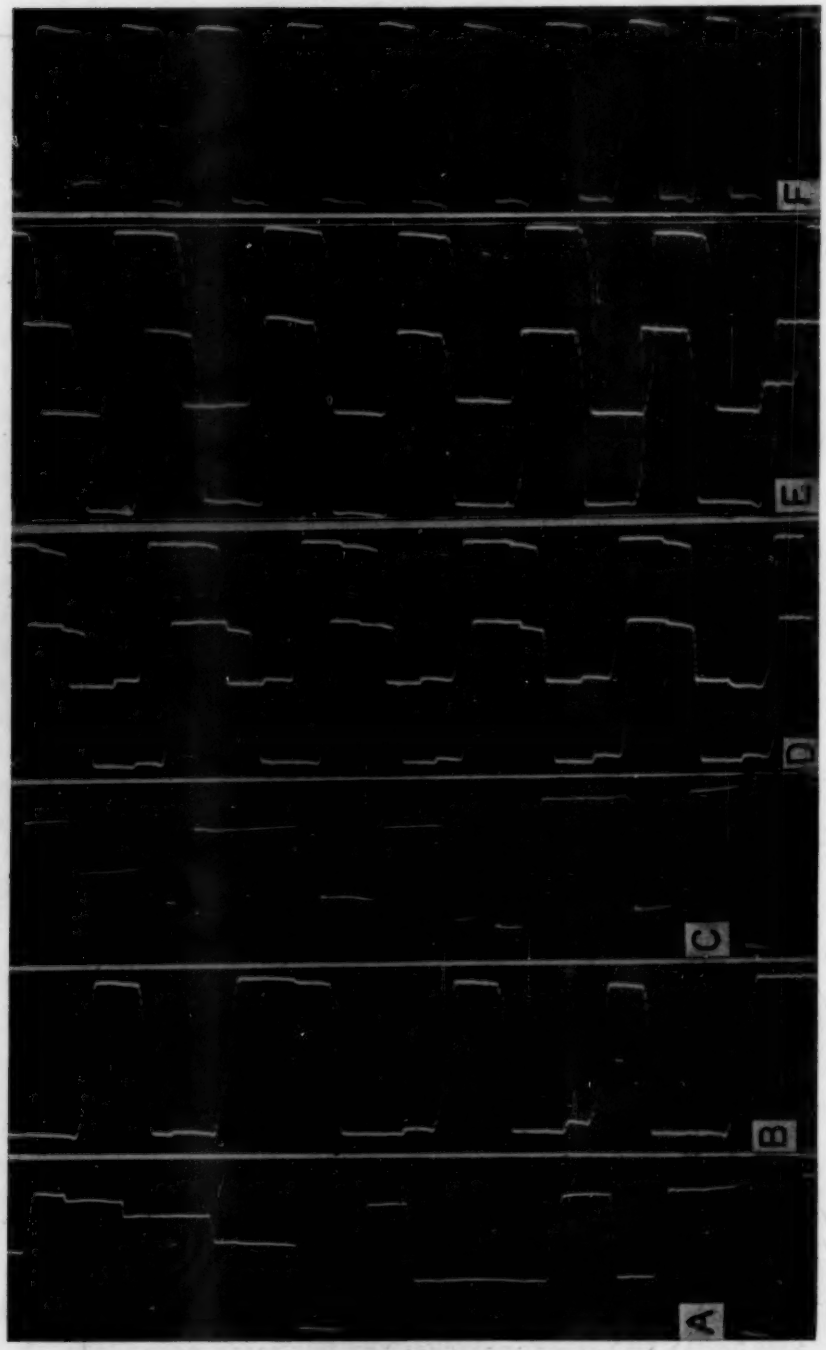


PLATE I. Sample records for different subjects showing the saccadic eye movements and fixation pauses when two points 40° apart were alternately viewed. The dots each have the time value of .01 second except in illustration D where they equal .004 second. All the records are to be interpreted from the bottom to the top. For description see text.

we have only the motion of the moving plate. Presently there is a sudden shift somewhat farther to the left, then a smooth vertical line which terminates in a sharp, almost right angle, turn to the right, and so on as we proceed from the bottom to the top of the record. In this record there were 250 interruptions of the light per second; therefore each dot with one interspace represents 1/250 second. If we wish to count dots and dashes separately each is equal to 2".

In the experiment recorded the subject looked back and forth keeping time with a metronome which was set at 80 ticks per minute. The instructions were to shift the fixation on the click of the metronome. In the record the vertical portions indicating the fixations are made of dots which fuse into each other. Had the plate been falling three times as fast the dots could have been counted in these sections also. Judging from the total vertical portions which separate the main movements, the subject in this case was successful in keeping up with the metronome.

The small arrows drawn on illustration D indicate the direction of eye-movement. It is obvious that a saccadic shift shows at first a rapid and quickly accomplished period of acceleration, a rather large arc of movement that is executed at a uniform maximum speed, and then a longer period of deceleration as the eye nears the new fixation point. Here we see again the features pointed out 25 years ago in Dodges' classical paper on the types of eye-movements (**6**). Dodge also drew attention to the fact that the eye in making movements between two known fixation points, does not as a rule quite reach the mark with the first or main saccadic. After the clearing up period, that is, when on the basis of retinal cues the subject realizes where he has landed, there is a further short saccadic to correct the fixation. Under the specific test conditions of looking alternately at given fixation points which are to be seen very distinctly, the corrective movement becomes an important criterion of conscious co-operation by the subject. Occasionally he can leap right on to the point with one saccadic jump. To speak in terms of golf, he can 'hole in one.' But ordinarily if he is giving visual

attention and looking sharply he requires 'two.' It is rather unusual for a subject to move too far and thus need to correct backward, as he did do at the very first of the second record of picture D.

The corrective movement occurs about 150$^{\sigma}$ after the eye has practically come to rest. This time interval is required for the subject to become aware of his visual position. It is a fundamental and essential reaction time; it cannot be reduced to zero. After correction there is what we may term a viewing interval. This can be prolonged but it cannot well be reduced below an amount equal to the visual reaction time value. The vertical section between two large movements in such a record tends to represent two reaction times, when the individual is participating with full consciousness of his visual experiences. Record D therefore represents a subject who is fully coöperating in the test. By itself it does not indicate whether he is in normal physical condition. But the form of the record proves that he is awake and coöperating; the count of the record would be used in connection with the time count on other similar records to measure the influence of whatever factor (drugs, diet or *x*) was being used.

In illustration *B* (Plate I.) we have a record showing poorer than average coöperation. Starting at the bottom it will be noticed that the first saccadic seems to have resulted in a 'hole in one,' for if we place a straight-edge along the corrected fixations on the left, this first fixation falls in line. The total time that the subject continued looking at the left mark was rather long; however, in this test a metronome was not used. The time wheel used in this case gave 100 interruptions per second which is the rate employed in all the other records shown, with the exception of D. The first movement to the right shows also an uncorrected fixation and here with the help of a ruler we see that the fixation fell short of reaching the true mark. The subject stopped long enough for the clearing up period, decided 'good enough,' or from inattention neglected to look sharply right at the mark and initiated a saccadic back to the left. At the end of this second move-

ment to the left [4] he required a rather large corrective shift to make the fixation accurate. Following the third abductive he corrected and saw the mark properly, or so we may interpret from the record. He always lined up quite accurately for the left fixation point, but was not so careful on the right. Illustration E which is a double record from another subject shows this latter tendency very well. He shows only one corrective movement in the entire two records and the fixations do not line up well vertically. In F, we have a record of exceedingly rapid eye movements. The speed is indicated by the obvious stretching out of the time dots. The subject apparently spent the least possible time in looking, in this case 'semi-fixating' might be a nearly correct description. It is important to note that even under a condition of maximal speed and of minimal attention to the marks the eye does not demonstrate a pendular motion as would be true of oscillating a finger or hand at maximal speed. The eye comes to a stop for an interval which we may assume is equal to the subject's reaction time.

Illustration *A* of Plate I. probably represents for the horizontal axis the eye movement behavior of an individual who is looking back and forth before proceeding over a street crossing. The points to be looked at are many, there is no specific direction to alternate fixation, but such a situation tends to call forth an alternating from right to left. Some of the fixations will be of short duration, others prolonged according as interest prompts. Since one is not trying for specific predetermined points but only to comprehend the general situation, the feature of the corrective shifts will tend to disappear. This particular record (*A*) is one where the subject did not understand that he was to alternate his fixations between the certain two dots. There were several other points that could serve for fixation and therefore he showed what we may suppose is typical survey behavior. During the first abductive he winked thus cutting off the

[4] All of the records shown in Plates I. and II. are from the right eye with the left eye covered. A movement to the left in any illustration is adductive while a change from the left mark to the right one is abductive.

light from the cornea by lowering the lid. It is visual economy to service the eye by a wink at the time of movement and this behavior is often found in connection with long reading records. The wink tends to occur at the moment of making the saccadic for changing fixation to a new line of print. The same subject made record *C*, but this was done about 3 hours after *A* and also after ingesting some alcohol. The entire day's experiment went through with the misapprehension about what he was to do. In *C* the behavior so far as fixation points are concerned was about like *A*. However, on inspecting them we see that the dots are smaller in *C*, showing slower speed; and the eye is closed several times. When the eye closes the record is intensified and turns back on itself showing that the eyeball is elevated at the moment of winking. When the eye opens again and takes up the process of active vision, the visual axis temporarily elevated while the lids are closed must be lowered and re-adjusted. This is indicated by the trailing off appearance at certain places.[5] In record *C* the fixations waver and drift about and finally the eye is closed. The entire picture is indicative of an individual who is anything but alert. He is paying scant attention to retinal impressions, he is slow and his eyes probably feel tired and uncomfortable, hence he keeps them closed for intervals of longer duration than in the winks of normal life.

IV

On the basis of our general analysis of eye movement records for alternate fixation of specified points 40° apart we may now examine the four tracings shown in Plate II. These photographs are all for the same subject. There are two pairs; 1 and 2 were made on one evening, while 3 and 4 were recorded on another night three months later. The experiment had primarily to do with typewriting. The subject was a capable typist and regularly employed in a responsible position during usual working hours. He was

[5] In another publication fuller description of the movements of the eyeballs in connection with winking will be given.

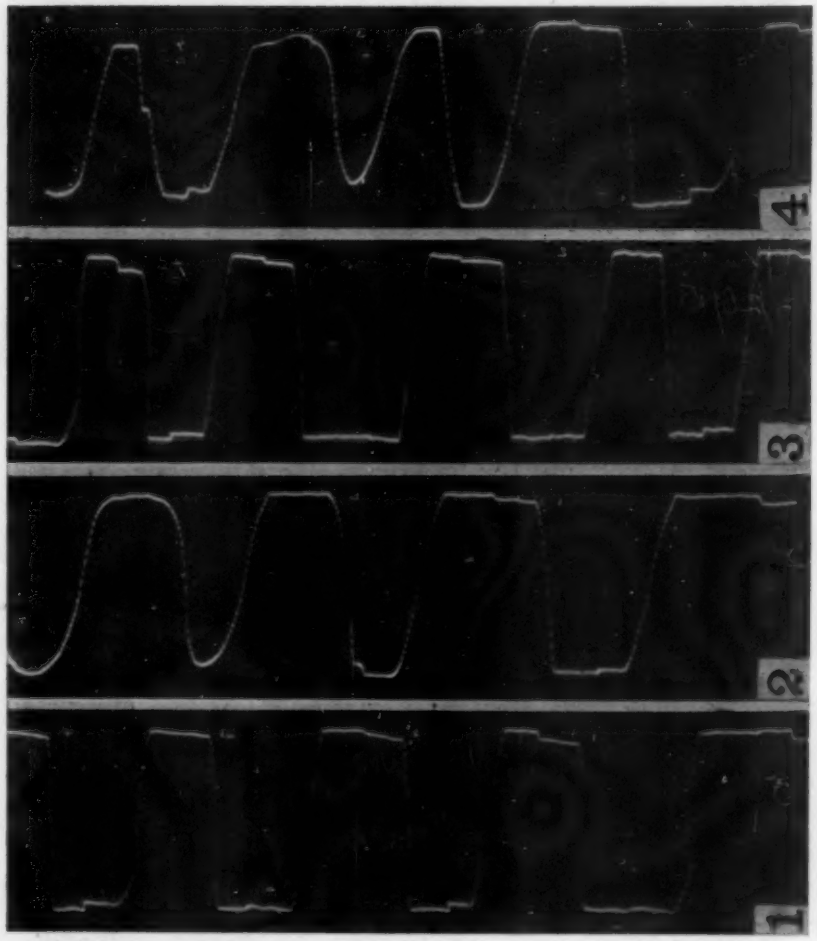


PLATE II. Eye movement records showing the change in type of movement with the onset of sleep while the eyelids were still open. Nos. 1 and 3 show slow movements but adequate fixations, the man is sleepy but not asleep. Nos. 2 and 4 show him awake when the records start (bottom), but lapsing into sleep before they finish 8 seconds later. In No. 4 he goes to sleep, rouses up and goes to sleep again all within 4 seconds.

anxious to serve in the evening experiments because of the opportunity thus given him to make some extra money. These circumstances are mentioned because they indicate that the man was really strongly motivated to do his evening work in a manner which would satisfy the investigator. He had agreed to a program in reference to hours of sleep, but certain social engagements, it was discovered later, caused him to make exceptions without notifying the experimenter. Therefore, it occurred that on these two dates the subject, having seriously robbed himself of sleep during two previous nights, found that he could hardly keep up with the work program. Record 1 was taken early in the evening. There are no winks shown and most of the fixations seem very satisfactory, particularly 2 and 4 on the left and 1, 2, and 3 on the right. The entire record compares well with *B* in Plate I., except that the movements are much slower. The distance to be covered was the same in both cases. The somewhat wider total record than is given by other subjects is due to the fact that this man has a smaller corneal curvature. This feature is however, not important to the present discussion. It must be understood that the subject professed to be in good condition for the experiment, and so stated at the beginning of the session; he was therefore well motivated to combat his sleepiness.

Record 2 came 3 hours after No. 1. When the photographic plate was released and the signal given to start, a shield carrying a preliminary fixation point was suddenly dropped. This turned the photographic light on the eye and exposed the right-hand fixation mark. The record shows that the subject moved over to the exposed mark one reaction time after it came into view. He looked at this mark for one second or more and then started to move his eye to the left-hand mark. It will be seen that the movement is fairly direct but is very slow. So far as I know this is the slowest saccadic movement, between two apparently adequate fixations, that is on record for normal adult subjects. The time is about 250″, more than twice as long as the usual time required for this visual-motor act. The resulting fixation

was apparently adequate and steady. The next abductive movement was considerably quicker (following the law of faster abductive than adductive movements) but much slower when compared with the corresponding portion of record 1. The fixation shows some wavering. Following this is an adductive, the second one from the bottom, extremely slow as it terminates, which probably carried the eye a little beyond the left mark. After a period suitable for clearing up the visual impressions there is what resembles a corrective shift back, but the period of fixation following this corrective is an oddity in such records. Usually after correction there is fixation for a period at least as long as one reaction time. Here it is much briefer, shorter indeed than any of the pauses in record *F*. It seems possible that with very slow saccadics the angular rotation rate, as the movement terminates, might allow fair vision so that corrective shifts could be dispensed with. This of course would assume that in such cases the elaboration of the impulse to move the eye is not fully completed, on the basis of the peripheral and attitude cues, while the eye is at rest on one fixation mark. Such an interpretation assumes a supplementing neural discharge into the motor paths during the actual occurrence of the movement. At the present time we can hardly do more than raise the question. The next abductive, the second main abductive from the bottom, does terminate very slowly and no correction movement appears. Furthermore, the fixation gives evidence of having been at the correct position, for the first three fixations at the right of the picture are well in line. The third fixation at the right terminates by a moderately sharp turn to the left initiating the next adductive. Now occurs something that, so far as I know, is new in the description of saccadic eye movements: the movement converts itself into a pendular swing or roll of the eye. There is no steady moment of fixation, no holding the eye still, on this third occasion when its visual axis is turned to the left mark. It seems justifiable to assume that just prior to reaching the position approximating the left hand mark the subject, to use Bell's expression

'ceased to have speculation.' The eyes continue to move for a time under stimulation of the regular activity that the muscles have just been engaged in, and probably also the subject is governed by the fact that the experimenter is calling off in a subdued voice, 'left, right, left, etc.' But since vision has ceased, or practically so, and the visual centers are sleeping, the pattern of the movement changes distinctly and the eyes are simply rolled back and forth as if following some slow ponderous object.

The second pair of records, that is, Nos. 3 and 4 of Plate II., are so nearly a repetition of the experience three months earlier that they need not be described in detail. No. 3 corresponds quite well with No. 1; both were taken when the subject was at his best on the respective evenings. In No. 4 the corrective fixation is shown at the start. The subject winked during the time of executing the first adductive. The fixation at the left was evidently adequate although somewhat wavering. The first large abductive began and ended with fairly sharp corners, which is another way of saying that it was reasonably rapid and there was a corrective movement following. The next movement to the left (second adductive from the bottom) is initiated very slowly and terminates still more slowly—a very crawl of the recti on all fours—with the result that there is no correction required or at least used although there is some indication of fixation. The eye now creeps over to the right. It is not certain that this fixation is accurate, probably the eye came to rest somewhat to the left of the true mark. It mattered little to the individual in question for his visual centers were just ceasing to be awake and the eye muscles take up the rolling type of action for one cycle, which carries the eye to the left and, then without pause, slowly returns it to the right. There was a developing change of status of retinal impression while in the vicinity of the right-hand mark; the curve changes its shape and we may reasonably interpret this to mean that the visual centers are to some degree again awake. The large movement next recorded is started and stopped more promptly than its immediate predecessors, a

corrective movement is followed by fixation, evidence that the retinal impressions were functioning. The next cycle (movement to the right and back to the left) was executed in a more depressed condition, and near the end of it the visual centers were apparently again asleep, for the eye begins to roll and now the lids close. Illustration No. 4 thus gives evidence of the subject's having gone to sleep twice during the 8-second period represented by the photograph.

The last abductive movement claims our attention because of an extremely short vertical portion that occurs about 3/5 of the way across. There is no other way for this mark to have been made upon the record except by a short pause of the eye. It is noticed that beyond this point the eye moved more rapidly as evidenced by the fact that the dots are longer. What we have then is really two abductives used consecutively with a stop sandwiched between, that is obviously much too short to serve as a clearing up period. At present we must content ourselves with simply calling attention to this oculo-motor phenomenon; it is so unusual that any present discussion of it would be the merest guesswork.

V

It may be just a coincidence, but it is at least worth noting, that each time when the individual has definitely lost consciousness of the retinal impressions the condition has come on during the adductive saccadic movement, that is, when the eye was being shifted from the right mark to the left one. We know that the adductive is slower than the abductive. For the individual subject under discussion the adductive movement seems to offer an easier opportunity for the eye to lapse into the slow rolling which is shown to persist even after visual consciousness has ceased. In other connections I have observed that the adductive is more sensitive to the effects of single doses of alcohol and to the influence from undernutrition than is the abductive saccadic.

Securing photographic records at the moment of going to sleep is in my experience a matter of rare chance. If I try to arrange for such an occurrence the individual sensing that

I am willing to have him go to sleep seems either inhibited from doing so, or else closes his eyes and so cuts off the possibility of photographic recording. It is only when the subject, trying extremely hard to keep awake lapses in spite of his efforts, into moments of unconsciousness, that we find the fleeting opportunity to catch him in the act. The situation exhibited by our two sleep records may represent fairly well behavior like that of a very sleepy man driving an automobile. In general the individual realizes under these conditions the great importance of remaining awake, he alternates fixation from one point to another upon the road before him, but in some cases there come moments when he is quite unconscious of the retinal impressions although the eyes may be open. The sudden flash of an on-coming light or the nod of his head as motor centers cease to be active brings him again to awareness, and often to fright. A person sitting on the front seat with him might later testify that the driver's eyes were open ('and he therefore was awake') just prior to the collision.

An individual may be well motivated for the reading in which he is engaged and may yet fall asleep in spite of all his efforts. Frequently after such a nodding off one finds on coming again to awareness that the eyes are directed to a portion on the page which is perhaps several lines below where understanding consciousness lapsed. I have no record where a person has gone to sleep during the photographing of reading eye movements, but if we may assume our records 2 and 4 of Plate II. as typical, such events probably mean that in falling asleep generally, the eyes cease to show steady pauses for fixation and the saccadics drag into a vagrant roving roll somewhat like an automobile travelling without the control of a driver.

For the last 100 years it has been pretty well agreed that during sleep the eyes are rolled upward. Anyone can verify the conclusion, on a child sleeping soundly, on an adult during anesthesia or during a fainting spell. The eyes are not only seen to be in general turned upward but they roll slowly back and forth. Sometimes this slow movement can

be observed through the closed lids. We may call this the rolling eye movement of sleep. It or something similar may occur in connection with some emotional situations. Movements of the sort appear to be typical for the very young infant before fixations of the eyes are common. These horizontal eye movements which we have found to occur also at the very onset of sleep resemble pursuit movements when the eye is closely following some object like a slow swinging pendulum. Dodge (**6**) and Öhrwall (**10**) have held that the typical pursuit eye movement requires an actually moving object to serve as fixation point. It seems in general true that when the moving point disappears the smooth pursuit movement at that moment ceases, and a typical saccadic is made to the point where the person expects the object to reappear. When the reappearance takes place the pursuit is again reinitiated. If the object moves too fast the pursuit will now and then be supplemented by saccadic shifts to correct the fixation. Gertz (**11**) has shown that for himself at least, it is not absolutely necessary to have a moving point seen at the fovea. He can get pursuit movements and maintain them on the basis of peripheral moving cues. Furthermore, Gertz has shown that in his own case under certain circumstances, especially when in a moving train, he induces pursuit, or as he calls them 'gliding' movements without having either macular or peripheral moving stimuli. However, at least for adults who are awake and with their eyes open, the pursuit movement is none other than an adjustment for seeing a moving object and is otherwise out of place. If it occurs without such a moving stimulus, and providing the person has not developed some eye movement stunt, it appears from the records here presented, that a lapse of consciousness has occurred.

These data on the onset of sleep are in harmony with our general observation. Muscular activity seems able to continue for some time after consciousness has lapsed. The soldier on the march falls asleep but continues to walk, the farmer hoeing arouses to find that he has struck once or twice without knowing what he struck at, the auto driver

continues to hold the wheel, or the air pilot the stick, and a person reading aloud may haltingly speak a few words after the visual centers cease to be active. Instances might be multiplied; the motor apparatus may continue functioning or activity may even be initiated in it during sleep. The visual areas rather than the oculo-motor nuclei are first to succumb.

Summary

1. The contrast between alertness and drowsiness is most evident in the behavior of the eyes.

2. Sleep is usually immediately accompanied or preceded by closing of the eyelids, but it is possible for the onset of normal sleep to occur with the eyes open.

3. In the normal waking state when the individual is attentive to retinal impressions the eyes execute rapid movements (saccadics) back and forth and at the end of each such movement there is a pause for fixation. The fixations are from 3 to 10 times as long as the duration of the movements.

4. An eye movement test which requires the subject to look back and forth at two dots placed in the horizontal and 40° apart shows (by means of photographs) certain features: normal speed of such movements, duration of the clearing up period, corrective fixation movements, and duration and steadiness of fixations, which may be used as criteria indicative of the waking state.

5. The speed of saccadic eye movements is found to be much slower when a person is sleepy.

6. In two records which are reproduced, a subject, although strongly motivated to remain awake, went to sleep during the eye movement test. The eyes remained open for a few seconds, the typical saccadic movements which had been very slow now changed into a rolling movement gliding back and forth as if following a slow pendulum, and all evidence of fixation disappeared.

7. In each of three instances the onset of sleep came during the adductive movement.

8. The eye muscles are shown to remain active after the visual centers have ceased to function through the retina.

REFERENCES

1. POSEY, W. C., AND SPILLER, W. G., 'The Eye and Nervous System,' Philadelphia, 1906, see p. 692.
2. BELL, C., 'Anatomy and Physiology of the Human Body,' New York, 1827, 5th Amer. Ed., Vol. II., p. 215.
3. SPENCER, SIR BALDWIN, AND GILLEN, F. J., 'The Arunta,' London, 1927, II., p. 431.
4. DODGE, R., AND BENEDICT, F. G., 'Psychological Effects of Alcohol,' Carn. Inst. Wash., Pub. 232, Washington, 1915, see pp. 150–167.
5. SHEN, E., 'An Analysis of Eye Movements in the Reading of Chinese,' J. EXPER. PSYCHOL., 1927, 10, pp. 158–183.
6. DODGE, R., 'Five Types of Eye Movements in the Horizontal Meridian Plane of the Field of Regard,' *Amer. J. Physiol.*, 1903, 8, pp. 307–327, see p. 312.
7. MILES, W. R., 'Alcohol and Human Efficiency,' Carn. Inst. Wash., Pub. 333, Washington, 1924, see p. 154.
8. BENEDICT, MILES, ROTH, AND SMITH, 'Human Vitality and Efficiency Under Prolonged Restricted Diet,' Carn. Inst. Wash., Pub. 280, Washington, 1918, see pp. 616*ff.* and 637.
9. DODGE, R., 'Elementary Conditions of Human Variability,' New York, Columbia University Press, 1927. Pp. 107.
10. ÖHRWALL, H., 'Die Bewegungen des Auges während des Fixierens,' *Skan. Arch. f. Physiol.*, 1912, 27, pp. 304–314.
11. GERTZ, H., 'Über die gleitende (langsame) Augenbewegung,' *Zsch. f. Sinnesphysiol.*, 1914, 49, pp. 29–58.

[MS. received July 6, 1928]

FACULTY PSYCHOLOGY

BY CARROLL C. PRATT

Harvard University

In the early history of philosophical speculation the human mind was frequently conceived of as possessing one or more powers or faculties. Plato, *e.g.*, believed that the mind had two separate capacities: that of Sense, which produces only appearance, and that of Intellect, which affords knowledge of reality. The subsequent ramifications of psychological speculation had pretty generally their point of departure in postulates concerning a few fundamental powers of the mind. These postulates eventually crystallized into the famous triumvirate of Intellect, Feeling, and Will, a formulation which still holds sway under the terms cognition, affection, and conation. But in the hands of Ch. Wolff, and the later phrenologists, the doctrine of faculties fell into disrepute, and the pride and delight which Herbart took in dealing it what he considered a death-blow is reflected today in almost every textbook of psychology in those sections treating of theories once held by psychologists but now discarded. One would suppose from a perusal of these texts that psychology reached its lowest depths of degradation in the eighteenth century and that the chief reason for rejoicing and congratulation at the present moment is that the science no longer has traffic with mental activities, capacities, or faculties. A few quotations, selected at random, will illustrate this attitude. "In the history of psychology a good many attempts at analysis have taken a wrong course. To come to the most pernicious of these we pass over the reduction of experience (common also in the layman's way of thinking about himself) to the faculties or powers of 'knowing,' 'willing,' and 'understanding,' to the 'mental organs' of the phrenologists, and

to the stereotyped bits of knowledge which the traditional English psychologists called 'impressions' and 'ideas.'" [1] "The whole analysis of faculties was vague and erroneous from a psychologic point of view." [2] "The so-called 'faculties' of memory, imagination, attention, and reasoning . . . are gross, unanalyzed terms of a popular psychology . . . ." [3] "Both the older and the later form of faculty psychology have long been discredited." [4] "Zwar kann auch die heutige Psychologie den Vermögensbegriff nicht entbehren, denn obwohl man heute eine gewisse Scheu vor dem Wort hat, so bedeutet doch der vielgebrauchte Ausdruck 'Disposition' tatsächlich dasselbe. Aber man ist sich wenigstens im allgemeinen darüber klar, dass mit der Zurückführung eines Vorgangs auch ein 'Vermögen' noch keine wirkliche Erklärung gegeben, sondern nur das Bedürfnis nach einer solchen ausgedrückt ist." [5] "This opinion (*i.e.*, the formation of connections by 'faculties') is vanishing from the world of expert thought and no more need be said about it than that it is false and would be useless to human welfare if true." [6] "We may say in criticism (of faculty psychology): (1) that the faculty names are merely classificatory concepts; and that the subsumption under them of the ideas, feelings, impulses, *etc.*, which are really given in introspection, does not help us in the least degree towards an understanding of these processes. A faculty psychology must, that is to say, be at best a merely descriptive psychology, and can never rise to the level of explanation. (2) But further, the faculties, which as class-names are products of scientific abstraction, become changed in the faculty-systems into actual forces or powers, which are supposed to give rise to the separate ideas, feelings, *etc.* In other words, the faculty which, rightly defined, is incapable of affording explanation, is substantialized, and so made the ground of a wrong explanation.

[1] Bentley, M., 'The Field of Psychology,' p. 35.
[2] James, W., 'Principles of Psychology,' Vol. I., p. 28.
[3] Ladd and Woodworth, 'Elements of Physiological Psychology,' p. 274.
[4] McDougall, W., 'Outline of Psychology,' p. 13.
[5] Messer, A., 'Psychologie,' p. 12.
[6] Thorndike, E. L., 'Educational Psychology,' Vol. I., p. 174.

The first criticism charges the faculty psychology with impotence; the second charges it with seeking by false pretences to conceal its impotence." [7] In defining the will the psychologist might "refer to the old division of the mind into the 'three great faculties' of intellect, feeling, and will, but would be in duty bound to add at once that this 'tripartite division' is now regarded as rather useless, if not misleading." [8] "It was left for Herbart, one of the acutest thinkers of our century, to give a convincing proof of the utter emptiness of this 'theory' (of mental faculties)." "Nachdem man die Unbrauchbarkeit der Vermögensbegriffe gegenwärtig fast allgemein anerkannt hat, ist nun gleichwohl eine Nachwirkung dieser Auffassung noch weit verbreitet." [9]

In the adverse criticisms directed against faculty psychology it is not always apparent just what the trouble was with it. Many of the disparaging remarks seem to have been included in the texts merely because it is the fashion to congratulate psychology on its deliverance from former evils. I suspect that the force of tradition, so overwhelmingly powerful in social life and the arts, is not altogether absent in science. At the present time, *e.g.*, it is the fashion, in musical criticism, to call Brahms the great romanticist. Ten years ago the music of this same Brahms was generally looked upon as cold, intellectual, severely classical. So in psychology it is now quite the thing to do to call faculty psychology a relic to be viewed in the museum without bothering to justify its place among the relics. From my own perusal of the criticisms of faculty psychology I gather that there are at least two positive objections to the doctrine. One is that to postulate faculties is to exchange the hard-earned facts of description for the easy and fragile advantages of explanation and interpretation. I shall return to this objection later.

A second type of objection charges faculty psychology with being nothing more than the rationalizing and organizing of the layman's view of mind. It is popular psychology, the

[7] Titchener, E. B., 'Experimental Psychology,' Vol. I., pt. II., p. 188.

[8] Woodworth, R. S., 'Psychology,' p. 523.

[9] Wundt, W., 'Human and Animal Psychology,' p. 4; 'Grundzüge der physiologischen Psychologie,' 6th ed., Vol. I., p. 12.

psychology of common sense, like all doctrines of mind which have any traffic with purpose, striving towards an end, logical meanings and values. If there were no more to this second objection than the mere flouting of common sense in a scientific atmosphere it could be dismissed peremptorily. To dub a psychology worthless because it derives from common sense is no criticism at all. It is an attitude which has many points in common with the proverbial ostrich with its head in the sand. Nay, worse: the ostrich does rid himself temporarily of the sight of the hateful object, whereas the scientist keeps the objectionable doctrine in view but suffers from the delusion that he has rendered it harmless by attaching a name to it. In like manner whole nations have been conveniently removed from respectable society by being dubbed Huns or Bolsheviki. But the objection has a more subtle aspect, and for that reason its influence is more insidious.

Running through the literature of existential psychology [10] one finds frequent expressions of aversion to the introduction of views which are cast in the world of practical values, to applied as contrasted with pure or theoretical psychology. When the views are frankly practical and are designed for the solution of problems of human engineering the aversion is not so strong. There is nothing illegitimate, surely, in devoting one's energy frankly and openly to applied psychology. The following passage from Freud causes no grave alarm to the existential psychologist: "From the first of these standpoints, the *dynamic* one, psychoanalysis derives all mental processes (apart from the reception of external stimuli) from the interplay of forces, which assist or inhibit one another, combine with one another, enter into compromises with one another, *etc.* All of these forces are originally in the nature of *instincts;* that is to say, they have an organic origin . . . ." [11] The relation between the inter-

[10] I use the phrase *existential* psychology to denote that type of psychology which has developed from the Wundtian influence. Its characteristics have been described in a recent book by H. P. Weld, 'Psychology as Science,' 1927, 50–71.

[11] Freud, S., 'Psychoanalysis: Freudian School,' *The Encyclopædia Britannica* (13th Ed.), 31, p. 253.

play of energies and forces in this Freudian dynamic psychology and the concepts of faculty psychology is apparent enough. But, psychoanalysis is an applied psychology intended to deal with the mind of common sense. It is only when Freud suggests that his contributions are of importance to general theoretical psychology that one notes perturbations among the orthodox. Freud's whole system of thought, however, is so foreign to the existential psychology of the Wundtian sort that it gives little opportunity for waging battle on common ground. Hence Freud has escaped most of the shafts of the Wundtians which have been directed against psychotechnology. The force of this second objection to psychology which deals with the *conduct* and *use* of mind is seen more clearly when it is raised against systems which purport to be theoretical but which are said to be contaminated in reality by a point of view quite different from that accepted by psychologists who study mind scientifically for its own sake. Such a system of psychology is to be found in Brentano's *Psychologie vom empirischen Standpunkte.* The psychology of Wolff with its concepts of knowing, willing, and feeling, deals with powers and faculties. The psychology of Brentano, which is also of philosophical and scholastic origin, treats of mental *acts* as the unique characteristic data of mind. The possibility of drawing an analogy between the two systems is obvious. In an address delivered at the time of the dedication of Eno Hall, Princeton University,[12] Titchener spoke of three influences which had been harmful to the progress of experimental psychology.[13] The first of these influences was Herbartian psychology,[14] the third was the hostility of philosophy,[15] and the second and by far the

[12] Titchener, E. B., 'Experimental Psychology: A Retrospect,' *Amer. J. Psychol.*, 1925, 36, 313–323.

[13] Occasional reference will be made in this paper to the writings of the late Professor Titchener. I hope that the unfavorable interpretation given to these particular references will not be understood as an attempt to underestimate the importance of the works from which they are taken. I have singled out Professor Titchener's views, rather than those of Wundt or the earlier Külpe, simply because of their greater familiarity and accessibility to American students of experimental psychology.

[14] *Ibid.*, 314*f.*

[15] *Ibid.*, 321*f.*

most pernicious and harmful was empirical psychology as represented in exemplary form by Brentano's masterful little book.[16]

"The second unfavorable influence which I have to note is of far greater importance—partly because of its antiquity and perennial vitality, partly because of its seeming innocence and the guise of kinship with experimental psychology which it often assumes, so that it works insidiously from within rather than openly from without. It is the influence of empirical psychology, a mode of psychology that goes back to Aristotle and Thomas Aquinas, and that forms the staple contents of most psychologies, down to and including our twentieth-century textbooks. The empirical psychologist professes to take mind as he finds it. . . . And, like everybody else, he finds mind in use, whether in man's converse with himself or in his intercourse with his fellow-men. This subject-matter, then, mind in use, the empirical psychologist seeks to reduce to order and arrangement; and empirical psychology thus becomes a rationalization of practice, a 'description and explanation' of mental uses. . . . Brentano starts out from mind in use, and his general psychology is, one might almost say, a theoretical applied psychology; it is, at any rate, a rationalizing of general mental application." [17]

In all seriousness I must confess that the central theme of the above objection to empirical psychology strikes me as a most amazing attitude. The question, in the present context, is not one of the exact meaning of the word empirical. Let it be granted that the meaning of the word empiricism as used by Brentano has been correctly interpreted.[18] The crucial question relates rather to the supposed danger that results from an empirical approach to the study of mind. An added note of warning is given by Weld. "Empirical

[16] *Ibid.*, 315–321.

[17] *Ibid.*, 315*f.*, 319*f.*

[18] The proper definition of the word empirical as applied to psychology, especially Brentano's, has given rise to interesting controversy. *Cf.*, in this connection, Carmichael, L., 'What is Empirical Psychology?,' *Amer. J. Psychol.*, 1926, **37**, 521*ff.*; Boring, E. G., 'Empirical Psychology,' *ibid.*, 1927, **38**, 475*ff.*; Ruckmick, C. A., 'Empirical Psychology,' *ibid.*, 1928, **40**, 166*ff.*; and Titchener, 'Empirical and Experimental Psychology,' *J. General Psychol.*, 1928, **1**, 176*f.*

psychology . . . does not distinguish between scientific facts and those of everyday life. . . . It is easy to see that it is not science in the critical sense. . . . Empirical psychology is a theory of conduct; a theory of mind in use." [19] Now, I should like to ask, what kind of mind is there which is not in use? The empirical psychologist takes mind as he finds it. Well, again, if you don't take mind as you find it, what, pray, do you take? To ask such questions suggests immediately rather absurd answers. But they are answers which follow strictly, so it seems to me, from the argument which the existential psychologist makes against empirical psychology. And I venture to suggest that the existentialist, following rigidly the lines of his own logic, frequently pursues a type of mental phenomena which, because of their high degree of abstraction and forced artificiality, lead ever further and further away from data which permit of fruitful deductions. Luster, bright and dull pressures, glassy sensations, greys that are preperceptual and somehow lively and pressury, morenesses and lessnesses which are not quantitative—such are the descriptions which not infrequently issue from the existentialist's point of view towards mind. I mention them in no spirit of pleasantry. On the contrary, I have tremendous and solemn respect for the sharpness of observation and the patience with experimental technique which such studies reveal. And I do not doubt for a single moment the accuracy of the results. But I do doubt very strongly their importance for the science of mind. Not all that which is delivered up by introspection is of equal significance. The psychologist, like every other scientist, must pick and choose what he is going to observe, so that the results will throw light on important unverified hypotheses. It is difficult to see in just what way these observations (fascinating as they are in and of themselves) put psychology ahead. They derive from minds in use, a very particular sort of use, but as reported in the literature [20] they seem of little use in understanding the workings of mind.

[19] *Op. cit.*, 43, 42, 86.

[20] Bixby, F. L., 'A Phenomenological Study of Luster,' *J. General Psychol.*, 1928, 1, 136*ff.*; Nafe, J. P., 'An Experimental Study of the Affective Qualities,' *Amer. J. Psychol.*, 1924, 35, 507*ff.*; Möller, E. F., 'The "Glassy Sensation,"' *Amer. J. Psychol.*, 1925, 36, 249*ff.*; and the introspective reports in these and similar studies.

If the existentialist's chief objection to empirical psychology, particularly Brentano's, were the doctrine of act or the dictum that characteristic of all mental phenomena is their reference to an object, the grounds for debate would be clear enough. But to put empirical psychology aside because it deals with mind as it is found in use is like lodging a complaint against the physicist for introducing the resistance of a circuit into his calculation of electromotive force. This extreme form of antagonism towards empirical psychology, however, is only a tendency written large which manifests itself here and there in psychology, particularly that which Weld calls existential, against those doctrines which attempt to systematize meanings, values, purposes, motives, capacities, abilities, and the complex behavior of human nature. In many textbooks these phenomena are almost completely ignored; in some they are treated perfunctorily and then dismissed; in a few, efforts are made to justify excluding them or treating them briefly. Although one could probably find, by searching, several reasons given for slighting these phenomena, my impression is that most of the reasons boil down to variations on a single theme, *viz.*, that these phenomena are not palpable and are therefore not the subject of science, or that in some way or other they refuse to be subjected to experimental observation. "The world of science is that world about us which is visible, audible, palpable, *i.e.*, it is a perceptual, an observable world. . . . The 'workings of one's own mind' are not, in any strict sense, directly observable; they are meanings or interpretations. That mental activities and meanings cannot be observed is recognized by several authors." [21] Insofar as this theme derives from the same line of argument as that adopted against the empiricism of Brentano it may, I think, be ignored. But the shift of emphasis to observability and palpability presents a new phase.

Without denying that the final court of appeal in matters of scientific truth lies in some form of observation, it is still possible and desirable to insist that observation is only a

[21] Weld, H. P., *op. cit.*, 12, 44.

part of scientific methodology. In the writings of the Wundtian psychologists, however, observation is frequently raised to a position of supreme importance. After a discussion of the unfortunate influence of common sense in psychology Titchener, *e.g.*, comes to a section entitled 'The Method of Psychology'[22] in which we read: "Is there a special psychological method, a peculiar way of working, that he (the psychologist) must adopt in his study of mental phenomena? The answer is No: his *method is that of science in general.* This method may be summed up in a single word as *observation.* All scientific description, all description that reflects a disinterested and impersonal search for fact, is got by way of observation. . . . Science is not called upon to 'explain' anything; description and correlation are the modern—and more modest—representatives of the 'explanation' that an older science looked for and professed to find." The method of existential psychology, says Weld, "is observation; . . . its problem, like that of critical science, is description. It does not attempt explanation except in so far as it subsumes observed fact under some law. The fact that 'red' undergoes qualitative change in time with steady fixation, is thus explained by the law of adaptation; but this law does no more than generalize what, under the stated conditions, happens to all visual qualities."[23] We are to believe, then, that the sole task of science is to observe and describe and correlate, and for the existential psychologist observation is ideally limited to what are called the palpables. "The presence of impalpables," says Nafe, "in a scientific subject-matter is, indeed—unless one is ready, with Brentano and the Würzburg School, to define science *ad hoc*—anomalous, if not contradictory."[24] (This can be written in the face of a science which tells us about electrons, protons, atoms, and ions.) As far as I can discover, the meaning of palpable, for the existential psychologist, may always be equated to sensory stuff. Weld is emphatic on this point. "In answering this question [as to the nature of existential psychology]

[22] 'A Beginner's Psychology,' p. 19; also p. 327.
[23] *Op. cit.*, 66*f.*
[24] *Op. cit.*, p. 507

we note first that all 'acts' and 'functions' have disappeared; there are no states of consciousness, no objects of which we are conscious. *There are only sensory processes. . . . Now they have become the sole subject matter of psychology.* . . . The 'red' which the existential psychologist describes is not 'his' 'red'; it is not his experience any more than the radiant energy which the physicist describes is 'his.' There is, of course, no reason why with a shift of attitude the meaning of 'mine' or of 'fire,' of 'anarchy,' of 'danger,' or any other meaning might not be put upon the given process; but the shift of attitude *takes the individual out of science* and into common sense; there is no 'mine' or 'fire' or 'anarchy' or 'danger,' in either psychology or physics."[25] It therefore follows naturally enough, since the sole method of science is observation, and since in psychology the observable stuff must be palpable, and since palpable is another word for sensory, and since a shift of attitude obliterates the scientific aspect of the palpable sensory material, that many meanings, values, purposes, memories, motives, recognitions, hopes, fears, aspirations, doubts, longings, interests, reasonings, volitions, *Bewusstseinslagen*, and what not, fall cleanly outside of psychology.

But to return to the question of observation: let me repeat that the final test of truth lies in some sort of observation. But the necessity for observation in science as the preliminary, as well as the final, step on the road to discovery is so patently obvious that mere mention of it becomes almost a platitude. To insist upon its exclusive importance, however, is not a platitude. Observation is only a part of scientific methodology. In Mill's Logic we find only a few paragraphs on observation, and these are inserted only to point out that the difference between observation and inference is extremely hard to detect.[26] In a recent book on scientific method by Ritchie we read in the middle of a long chapter on the validity of laws: "It is an attractive notion that in an investigation we should start with no presuppositions about the state of things to be discovered but

[25] *Op. cit.*, 64, 66. Italics are mine.

[26] Mill, J. S., 'A System of Logic,' Bk. IV., Ch. I.

with perfectly open minds and a single eye to the facts. There is a fine Baconian smack about it. One thinks of Darwin examining the facts for fifteen years (or whatever the period was) before framing his hypothesis. In fact it is all in the sound English tradition. Nobody can have more respect for the English tradition than I have, so that it must not be thought that I have bowed the knee to any continental Baal when I say that this is all nonsense. Darwin must have had some sort of hypothesis or he would not have known what facts to examine. There were millions of facts and he could not attend to them all. To have an open mind is not the same thing as to have a vacant mind. The vacant mind is like the bottomless pit; no amount of facts will ever fill it. What is absolutely necessary is that the investigator should not allow any hypothesis to give him a bias against the facts. Apart from this *the more hypotheses he has the better.* I expect Darwin in his account of his work was thinking of Newton's little joke, 'Hypotheses non fingo.'" [27] The only other reference in the book to observation is near the beginning [28] where Ritchie remarks that every observation is in the nature of an experiment, and then plunges into the technique of experimentation, the rules of analogy, inference, induction, deduction, the formation of hypotheses, theories, laws, *etc., etc.*

Similar ideas, together with the same paucity of reference to observation, may be found running through Poincaré's work on scientific method. "Si timide que l'on soit, il faut bien que l'on interpole; l'expérience ne nous donne qu'un certain nombre de points isolés, il faut les réunir par un trait continu; c'est là une véritable généralisation. Mais on fait plus, la courbe que l'on tracera passera entre les points observés and près de ces points; elle ne passera pas par ces points euxmêmes. Ainsi on ne se borne pas à généraliser l'expérience, on la corrige; et le physicien qui voudrait s'abstenir de ces corrections et se contenter vraiment de l'expérience toute nue serait forcé d'énoncer des lois bien

[27] Ritchie, A. D., 'Scientific Method,' 1923, 104*f*. Italics are mine.
[28] *Ibid.*, 21*f*.

extraordinaires. . . . Le ferme propos de se soumettre à l'expérience ne suffit pas." [29]

Those who attach supreme importance to observation might argue, in reply to Ritchie and Poincaré, that there can naturally be no objection to an hypothesis which is a mere generalization and mathematical correction of observed data, but that the path of science is littered with wrong hypotheses, wild hypotheses, and speculations that would make even a metaphysician blush, and that it is this state of affairs which is so disgraceful. But you will remember in this connection Mill's famous passage in which he reminds the reader that "an hypothesis being a mere supposition, there are no other limits to hypotheses than those of the human imagination; we may, if we please, imagine, by way of accounting for an effect, some cause of a kind utterly unknown, and acting according to a law altogether fictitious." [30] Since, however, hypotheses of this sort, as Mill hastens to add, would not have the plausibility of those which ally themselves with known laws, most scientists do not indulge in them, but prefer, instead, to keep some check on their flights of fancy. Hypotheses which, in the light of known facts, are clearly improbable die a natural death, and the mourner's bench is empty, save, perhaps, for the father of the deceased. Poincaré even goes so far as to suggest that a wrong hypothesis is more valuable than a correct one. "L'hypothèse ainsi renversée a-t-elle donc été stérile? Loin de là, on peut dire qu'elle a rendu plus de services qu'une hypothèse vraie; non seulement elle a été l'occasion de l'expérience décisive, mais on aurait fait cette expérience par hasard, sans avoir fait l'hypothèse, qu'on n'en aurait rien tiré; on n'y aurait rien vu d'extraordinaire; on n'aurait catalogué qu'un fait de plus sans en déduire la moindre conséquence." [31]

Without denying, then, that observation is important, it is still possible and desirable to insist that it plays a coördinate rôle with certain other procedures in scientific method. The observed datum is, of course, the first essential step. One

[29] Poincaré, H., 'La science et l'hypothese,' 1902 (rep. 1920), 169*f*., 179.
[30] *Op. cit.*, Bk. III., Ch. XIV., par. 4.
[31] *Op. cit.*, p. 179.

can't erect an hypothesis unless there is something to get hypothetical about. But once having made a preliminary survey of the field the next step is to allow one's scientific imagination to play about in the logician's games of analogies, hypotheses, theories, interpretations, and explanations.

The introduction of the words interpretation and explanation is, I realize, fraught with danger. In pointing out what is meant by *critical* science Weld argues that "the characteristic method of science is not induction but observation. . . . The problem of science, it is maintained, is simply to describe the world as science sees it. Science cannot explain events by assigning causes. It can only do the one, and cannot do the other, by virtue of its self-imposed limitations. It is restricted to description because it begins with observed fact, and because it regards no logical conclusion about observed facts as valid unless it can be verified by observation. . . . When science takes its attitude toward the whole of experience, facts result. When, on the other hand, common sense takes its attitude toward the same experience, appreciations, interpretations, values result." [32] And we have already heard Titchener saying that science is not called upon to explain anything; that description and correlation are the modern representatives of the explanation that an older science looked for and professed to find. Granted! But surely the burden of those sentences is largely linguistic, a more accurate definition, if you please, of the word explanation. Modern science is alive to the fact that a correct explanation turns out on examination to be the statement of uniform antecedents and consequents—correlations, in other words. When an astronomer states that the deflections in the orbit of a planet are due to the presence of a more distant planet he has pointed out a *correlation* between the two phenomena; he has *explained* the deflections. Which phrase one chooses to adopt is unimportant provided one knows what he means by the phrase. When the existence of the more distant planet was unknown the same explanation of the deflections was offered. In this case the explanation was

[32] Weld, H. P., *op. cit.*, 15*f*., 13.

an unverified hypothesis. And so long as scientists preserve a genuine curiosity about their subject-matter they will continue to offer explanations. But the defenders of so-called critical science look askance at explanations and interpretations. In psychology one of the frequent objections to the doctrine of faculties, as we have seen, is that it offers not a description but an interpretation of mind. The existential psychologist, in particular, objects to the introduction of interpretations into his field of observed fact.

"It seems, however, that the prime factor in *the common-sense notion of mind* is the idea of activity. We ascribe to mind the same sort of voluntary and purposeful activity that we ascribe to our fellow-men; and we distinguish this activity from the blind necessity of cause and effect. We find ourselves, and those about us, deliberating, intending, resolving, planning, recalling, doubting; and we say that these and similar activities are activities of mind. . . . So long as we stick to examples, all this seems straight-forward; only it is not easy to decide whether mind is activity, or whether these various activities are activities of mind. On the whole, common sense leans to the latter view: the activities are manifestations of mind. Mind itself is then something immaterial, lying behind the manifestations. What sort of thing? Apparently, another human being, *an inner man that dwells within the outer man*, an insubstantial mannikin living inside the head. . . . The inner man acts on the outer man and is acted on by him; but who or what gives the inner man, in his turn, the power to influence and to be influenced? We must suppose an endless nest of mannikins. That and other such arguments apart, however, the view is non-scientific because it offers an interpretation and not a description of mind. *The mind with which psychology deals must be a mind that is describable in terms of observed fact;* otherwise it cannot form the subject-matter of a science." [33]

It is evident that Titchener, in the above quotation, was trying to warn against the dangers of speculation by citing an interpretation which is palpably wild and absurd.

[33] Titchener, E. B., 'A Beginner's Psychology,' 6*ff*.

"Common sense," he says, "inclines to the view that mental activities are manifestations of mind." Agreed! "Mind itself is then something immaterial, lying behind the manifestation." Ah, but this is an interpretation which is completely out of line with probability and by no stretch of the imagination follows from the initial premise. There is nothing in the proposition, *a* is a manifestation of *b*, which would enable one to assert the immateriality or anything about the spatial localization of *b*. From the proposition as it stands one can predicate absolutely nothing of *b* except that it is a datum of which *a* is a manifestation. Similar remarks could be made, of course, about the further interpretative ramifications relating to the inner man that dwells within the outer man, the insubstantial mannikin living inside the head that acts on the outer man and is acted on by him. These views are unscientific, all will agree. But they are unscientific not because they offer an interpretation and not a description of mind, but because they offer an interpretation which is so utterly ridiculous.

All that anyone familiar with modern science means by an interpretation, I suppose, is an account of some hypothesis or theory which offers a reasonable guess as to what the missing items are in a group of data between the members of which correlations of one kind or another presumably obtain. No science that I know anything about has ever sought consciously to refrain from going across the threshold of observation into the hallway of obscurity and uncertainty. On the contrary, many scientists with restless and eager minds are constantly lured through the hallway into the black recesses of the metaphysician which lie beyond. At any rate, the life blood of science, so it seems to me, consists to some extent of the suppositions and guesses and wonderings over on the other side of observation. This speculative side of science lends to it much of its fascination and charm. The human mind, particularly that of the scientist and philosopher, has always been intrigued by the unknown. In psychology the unknown, as compared with the known, is so immense that the craving for theories and explanations is

more strongly felt there than in the other sciences. The craving is natural enough and, I am coming to believe, ought not to be curtailed in any way. Interpretations of mind which are plainly nonsensical, like the one quoted above, can be ignored. Those that are doubtful are, for that very reason, important, since, as Poincaré maintained, they give significance to facts which otherwise would stand isolated and meaningless. At one time it was a very plausible theory that eye-movement was responsible for apparent visual movement. When Wertheimer discovered that the optimal time for apparent movement is 60 sigma he was able to state categorically that the eye-movement theory was wrong, since the usual minimum time for eye-movement is 130 sigma. Without the hypothesis the fact of 60 sigma would have stood alone and isolated, but by reference to the hypothesis, and several others which Wertheimer examined, the fact acquired a halo of implications, so to speak, and became therefore very significant. The fact was more than a mere core; it was a core surrounded by a rich context. It is difficult to see how any fact can have meaning unless it explodes or cements a few hypotheses. The more theories, then, the richer will be the meaning of the facts. With too great emphasis upon observation there is just the bare possibility of a danger that the experimental technique which proved so profitable in the natural sciences will become a fetish when carried over to a science where as yet it is manifestly not so adequate. There are large stretches of mental stuff which still resist experimental attacks with a good measure of success. At any rate, theorizing and observation ought to go hand in hand, with, I suggest, theorizing always taking the lead.

The unfortunate results of too little speculation are not far to seek. Take the whole domain of sensory phenomena as worked over by the Wundtian type of psychology. Experimental observations were piling up till the shelves of libraries were creaking under their weight. But they tended to be isolated facts, for from them little could be deduced, on the basis of assimilation, colligation, and fusion, regarding the higher mental processes. And then Külpe invited his

scientific soul to a serious conference on the bad state of affairs, with the result that introspective psychology took a new lease of life with its *Aufgaben*, determining tendencies, and imageless thoughts. It may be that we are at another turning point just now with *Gestalttheorie*. Sensory phenomena are seen in a new light, new interpretations are giving psychologists pause. A still more striking example, perhaps, may be found in the field of intelligence. Surely many psychologists must have been oppressed by the regular meteoric shower of results which were accumulating from mental tests: a test for this, a test for that, one for Boston, another for San Francisco, a positive correlation here, a negative there, and the climax of indifference as to what the stuff all meant reached in Pressey's statement that although a large part of his time goes to work with tests of intelligence he is not very much interested in the question as to what he conceives intelligence to be.[34] But the situation is changing. The results of a test which formerly were of significance only in that they indicated what pupils in the sixth grade at Oshkosh would probably cope successfully with the work in the seventh grade, fit into the system of implications worked out by Spearman. This psychologist has made it possible for every tester to give his results an important theoretical interpretation if he cares to do so. Surely this one theoretical contribution outweighs in importance the countless results which were being fired into the journals by the machine guns of the testers.

Let us now turn to one or two phenomena in psychology which seem to have no satisfactory explanation. Spearman notes, in this connection, certain types of conflict and competition which occur in attention and other cognitive functions, and probably also in feeling and conation. It has long been matter of common observation that the number of objects clearly visible at one time is about six, or perhaps a few more, according to Fernberger's liminal determinations. The same limitation of attention is found in other sense

[34] Pressey, S. L., 'Intelligence and its Measurement: a Symposium,' *J. Educ. Psychol.*, 1921, 12, p. 144.

departments. Not more than about a half dozen tones or sounds can be distinguished at once or grouped successively; and tactile impressions apparently behave in the same way. As one leaves the sensory plane for that of ideas, analogous restrictions of the field of consciousness are found. The number of ideas which the mind may entertain at one time is always more or less definitely limited. The world stands in amazement before a Caesar who can dictate a letter and write another at the same time he is listening to the contents of a third being read to him. The store house of memory is enormous, a boundless chamber, but of this potential immensity only a tiny fraction is available at any one moment. It is quite possible that feeling and conation place similar restrictions on the field of consciousness. This widespread phenomenon of psychic limitation and competition has received little consideration at the hands of psychologists. They have generally been content to determine the quantitative boundaries of the limitation and let the matter rest there. But why cannot attributive or cognitive clearness encompass the whole universe? It seems to be taken simply for granted that attention is limited. Miss Calkins, *e.g.*, declares that "attention to part of one's total object of consciousness of course implies inattention to the rest." [35] But why, Spearman fairly shouts at her from the pages of his latest book, why in the name of all that is psychological this *of course*? Exactly! What is there about attention to one thing that implies inattention to others? Nothing in the observed facts points to any such necessity.

If I am listening to the violins in an orchestra and then quickly turn my attention to the oboes, the violins drop pretty nearly out of the stream of musical events. Why is that? It's certainly not due to the violins or the oboes; nor is it, apparently, brought about by anything intrinsic to the existential auditory qualities running along in consciousness. Spearman offers an explanation. He suggests that in the nervous system there is a mechanism, the operation of which regulates the amount of energy which can be put at the

[35] Calkins, M. W., 'A First Book in Psychology,' 1914, p. 107.

disposal of mental process. This energy is constant, or in ordinary situations roughly so, so that the range of attention, or ideas, or consciousness, cannot exceed the limits set by the nervous mechanism.[36] If this is true, then it follows at once that attention to one thing implies inattention to others; the entrance of one train of thought precludes the entrance of others, or if another comes in the first must go out; and the storehouse of memory can release only a few of its treasures at a time. The psychological side of the hypothesis Spearman calls the law of universal mental competition, which he assumes to be the generalization of observed fact. The physiological side is frankly hypothetical, but it goes far towards removing certain mysteries from mental life, and can easily be modified and altered, and if needs be, given up, as physiology advances.

Spearman uses the same hypothesis to account for the facts of intelligence which he has generalized in his two-factor theory. The general factor, *g*, is related to the energy or power which serves the whole cortex, and the specific factor, *s*, derives from the distribution of this energy into groups of neurones, or engines, as he calls them.[37] The theory of two factors is the embodiment of experimental and theoretical work in various related fields which has been accumulating for a period of some twenty-five years in Spearman's own laboratory and the literature. First, there is the statistical material from innumerable cognitive operations of all kinds and descriptions which, when subjected to mathematical analysis by means of intercolumnar correlations and tetrad equations, offers, Spearman insists, inescapable proof of the existence of *g* and *s*. Secondly, there are the qualitative laws of cognition under which may be subsumed all operations of knowing. These laws are three in number (apprehension of experience, eduction of relations, and eduction of correlates) and, with their ten corollaries, are adequate, according to the criterion of the tetrad equation, to the whole field of cognition. Coördinate with these laws are five quantitative laws (span, retentivity, fatigue, conative control, and pri-

[36] Spearman, C., 'The Abilities of Man,' 1926, 98–136.
[37] *Ibid.*, 74*f*.

mordial potency) which prescribe not the nature of cognition but the degree in which cognition of already prescribed nature actually occurs. Thirdly, there is the theory as to the nature of intelligence and the principles of cognition which, announced in rudimentary form twenty-four years ago,[38] has been the guiding principle in most of the work in the London laboratory ever since. But this is not the place to give even a brief summary of Spearman's psychology.

The most vulnerable point in the whole of Spearman's work is naturally the one which is most frankly hypothetical, *viz.*, the hypothesis of general mental energy. It is the region in which he has wandered furthest from experimental data and allowed his imagination freest play. The statistical findings themselves, I take it, are questioned by no one. Some of his interpretations of the mathematical treatment of these findings have been disputed by a few writers. But any one is at liberty to take a fling at his physiological guesses. But far from deploring this kind of controversy over speculation I am coming, rather, to look upon it as extremely wholesome for science, for it tends to keep the tool of reason sharp, whereas, given patience, the instrument of observation ought to work pretty well anyway. But if criticism becomes dull the results of observation lose their richness of implication. Psychology must take into consideration, as Professor Boring has clearly shown,[39] at least four variables: stimulus, nervous excitation, consciousness, and response. And the final chapter in psychology can be written only when all the relations between these four variables have been worked out. These variables, however, are not on the same footing with respect to what is known about them. For psychological purposes the variable which has divulged fewest of its secrets is the nervous system. And yet it may be argued that for a proper understanding of mind the correlations which are most badly needed are just those which obtain between either consciousness or response and some item in the nervous system.

Now it may be taken as almost axiomatic that where

[38] *Amer. J. Psychol.*, 1904, 15, 268*ff*.

[39] Boring, E. G., 'The Stimulus-Error,' *Amer. J. Psychol.*, 1921, 32, 465*ff*.

precise information is lacking speculation of one sort or another will step in to fill the gaps, unless some schooled aversion to such practice has developed. The neurological conditions which would undoubtedly throw light on many of the inexplicable phenomena of consciousness and behavior are thus far screened from view. One can only surmise what they are. In other words, it is absolutely necessary to indulge in speculative thinking and to construct hypotheses, unless one is content to remain at the level of observed fact. But the mind of the average scientist in the presence of an observed fact without a context of implication as to what is behind the fact is not unlike that of Brahms who, when a pianist stopped short in the middle of a composition, shouted, "For God's sake, play on. Don't let that chord go unresolved through all eternity." It is just possible that some psychological knots will not be loosened as soon as they might simply because, under the influence of an extreme existentialism, much psychological research stops short at observation so that subsequent observations are deprived of the advantage of a framework of hypothesis. Titchener, in the same address referred to above,[40] said of the progress of psychology, "We have, I think without question, passed from infancy into childhood. Our independence of physiology is a guarantee of that." Our arrival at adolescence will be assured when we are courageous enough to turn to phenomenology, a science of pure description.

Let us consider briefly the hypothesis of nervous energy.[41] The notion is not new, of course, with Spearman. One meets with it not infrequently in philosophical literature of an early date. Malebranche, *e.g.*, maintained that there is a constancy of mental power, and declared this power, in its totality, to be unalterable. The doctrine appears more or less regularly in scientific contexts in spite of the distaste

[40] *Amer. J. Psychol.*, 1925, 36, 322*f*.

[41] Spearman uses the term *mental* energy. It would seem more advisable to speak, rather, in terms of *nervous* energy. It is reasonable to assume that those mental phenomena which admit of certain characteristic quantitative formulations are, in some way or other, the product of nerve-mechanics, which, in turn, constitute a special case of physico-chemical relationships. To speak of *mental* energy is to introduce unnecessary metempirics.

for it which is occasionally voiced. It is interesting to discover, *e.g.*, that Fechner subscribed to the view that mental activity is the product of a system of energy continually consuming and renewing its supply. His historical position in psychology has been all too frequently confined to the section of his work on psychophysics. But the whole of the two volumes of the 'Elemente' is not devoted to metric methods and the Weber law. And then there are the two volumes on æsthetics, and the books 'Zendavesta' and 'Nanna,' which James found incomparably 'thicker' in ideas than the 'thin' metaphysics of Hegel. In the first volume of the 'Elemente' Fechner devotes over twenty pages to a treatment of the rôle of energy in mental functions.[42] "Der Wille, der Gedanke, der ganze Geist sei so frei er will; aber er wird seine Freiheit nicht *wider*, sondern nur auf Grund der allgemeinen Gesetze der lebendigen Kraft äussern können. Sofern sein Gang an den Gang der psychophysischen Tätigkeit gebunden und dieser an das Gesetz der Erhaltung der Kraft gebunden ist, wird er selbst dadurch gebunden sein." [43] Bain considers various views of nerve force and concludes that only in terms of physiological energy can certain mental powers be understood.[44] J. S. Mill maintains that "the best mode of singling out the operative circumstance (that qualifies the influence of frequency and repetition) is to describe it as 'Concentration of Mind'; the devotion of the mental forces to the thing to be done or remembered, the withdrawal of power from other exercises, to expend it on the exercise in hand. Every circumstance that at once rouses the mental and nervous energies, and keeps them fixed upon any subject of study or the practice of any art, is a circumstance in aid of acquisition. No fact more comprehensive, more exactly in point, can be assigned than the one now stated. What remains is to apply it in the detail, or to point out the occasions and conditions that favor, and those that obstruct, the concentration of the mental energy." [45] The extensive use

[42] Fechner, G. Th., 'Elemente der Psychophysik,' 1860 (rep. 1907), 21–45.

[43] *Ibid.*, p. 36.

[44] Bain, A., 'The Senses and the Intellect,' 1855, 57*ff*.

[45] J. S. Mill's notes to James Mill's 'Analysis of the Phenomena of the Human Mind,' Ed. of 1869, p. 118.

which Lipps made through his psychology of the concept of the psychic energy of mental process is too well known to quote. Neither is it necessary to quote more or less similar opinions of other psychologists, *e.g.*, Meinong, Jodl, Witasek, Höfler—in fact nearly every follower of the Brentano tradition; Breuer, Freud, Jung, Janet, Wirth, Höffding, Ribot, Ladd, James, Claparède, Lehmann, Heymans, *etc.*[46]

A somewhat detailed hypothesis of mental energy is offered by McDougall in numerous passages in his articles and books, the gist of which is perhaps given in the following quotation. "Unfortunately, our knowledge of the immediate effect of stimulation of a sensory nerve is still very imperfect; but there is much to be said for the view that the immediate and essential effect is a katabolic process which liberates chemically stored energy in the substance of the neurone, and that the spread of excitation consists in the discharge of this freed nervous energy from neurone to neurone across the synapses or places of junction of the neurones. This distincton beween chemically stored or potential nervous energy and the liberated active nervous energy is, I feel sure, one of the first importance for neurological speculation, although but little attention is commonly paid to it. Oscar Vogt has recognized its importance and has proposed to mark it by calling the freed nervous energy 'neurokyme.' Some years ago I, in ignorance of Vogt's work, proposed to call it 'neurin.' What name we use does not much matter, so long as we hold fast to this distinction and to this conception of liberated active nervous energy; but for this purpose some name is essential; since Vogt's proposal was prior to my own, and since my proposed name is so similar to neurine, the name of one of the chemical compounds found by chemists in nervous tissue, I adopt Vogt's term 'neurokyme' in place of 'neurin.'" [47]

Although the experimental evidence for the existence of a general supply of nervous energy seems not very abundant, not all physiologists refuse to entertain the hypothesis.

[46] References and comments may be found in Spearman, *op. cit.*, 117*ff*.

[47] McDougall, W., 'The State of the Brain during Hypnosis,' *Brain*, 1908, 31, p. 247.

Mott notes that the amount of energy liberated by a stimulus is not proportional to the exciting stimulus, and argues that the whole nervous system may be considered to be composed of physiologically correlated nervous units each of which has its *vita propria;* and that at the neuronic threshold the stimulus causes the liberation of stored energy which is always at the disposal of the nervous system.[48] And no less an authority than Sherrington insists that "one of the most helpful of the assumptions we can use in dealing with the problems of the nervous system and the diseases of the nervous system is that which regards the nervous system as more or less a reservoir of energy to be discharged, the charge depending very much on the condition of the reservoir itself; but it is very far from being a rigid system which simply conveys in various directions within itself the amount of energy conveyed to it by some peripheral stimulation."[49]

From the foregoing excerpts it can be seen that the hypothesis of a central supply of nervous energy is at least not an utterly wild one. It has been held by a long list of psychologists and by not a few physiologists. Nor is it a mere relic of speculation, for it was chosen as the topic of a symposium at the International Congress for Psychology in 1923, and attracted such eminent speakers as Adrian, Head, and Myers.[50] It is most emphatically not my intention to adjudicate in a matter so far outside my competence and knowledge. Nor do I propose to suggest how nervous energy gets itself related, mechanically, to mental phenomena. I should merely like to indicate the fruitfulness of such an hypothesis for the understanding of certain phases of mental life. The problems of the mechanics of the body-mind

[48] Mott, F. W., 'Importance of Stimulus in Repair and Decay of the Nervous System,' *J. Ment. Sci.*, 1902, 48, 667*ff*.

[49] *J. Ment. Sci.*, 1902, 48, p. 682.

[50] *Brit. J. Psychol.*, 1923–24, 14, 121–152. Speaking from a purely physiological point of view, Adrian thought the less said about nervous and mental energy the better, although he was ready to believe that the conception of mental energy might be of value to psychology. Head recognized the facts which make the assumption of a central supply of nervous energy plausible, and argued for an interpretation of these facts in terms of 'vigilance.' Myers supported vigorously the concept of mental energy.

relation or the liabilities involved by intercourse with any view bordering on interaction can be set aside in this particular case by following the correlational method of science, *viz.*, the simple statement of uniform antecedents and consequents. Given event *b* (which for convenience's sake may be called mental), the missing item in the neural mechanism to which it is supposedly correlated is called *a* (in this case nervous energy). Such a statement makes no further commitments than those involved in the assertion of an empirical relationship between *a* and *b*. Whether *a* or *b* is ultimately subjective or objective, mental or physical, need not be decided. They are rather the objects of certain kinds of scientific interests. That is all.

Now there are various phenomenological aspects of consciousness for which it is natural to seek correlates in the nervous system. Among these aspects I should most certainly include (1) all ideational material composed of sensory stuff in its various dimensions, (2) affective experiences, and (3) the acts of Brentano.

The neural correlates of sensory stuff, although not perfectly known, seem plausibly enough to reside in certain characteristics of nerve conduction. In vision, *e.g.*, the form of rhythmic modulation, the frequency of nerve pulses, and the degree of departure from the regular series of pulses, are supposed to lie at the basis of hue, brilliance, and saturation. Analogous relationships undoubtedly obtain in the other modalities. Following Troland, it may be assumed that affective experience is conditioned by rate of change of cortical conductance.[51] We are left, then, with mental acts to account for. But as soon as we take up the question of act we enter dangerous territory. Not only is it hazardous to attempt an interpretation of the act; it is hardly safe to assume that the act itself exists. Yet the majority of psychologists, I do believe, are either clearly or vaguely aware that there is a real problem confronting them in the act.

Psychology whi·h comes from philosophical, especially

[51] Troland, L. T., 'A System for Explaining Affective Phenomena,' *J. Abn. Psychol.*, 1920, 14, 376–387; 'Mystery of Mind,' 1925, 133*ff.*; 'Fundamentals of Human Motivation,' 1928, 287*ff.*

scholastic ancestry, the large and influential group of psychologists who in one way or another are related to Brentano, and pretty nearly the whole of British psychology recognize and accept the act frankly, and often consider it the central datum of mind. Other psychologists make impressive show of getting rid of the bugaboo of act, but nevertheless let it in by some circuitous concept like apperception, attention, *Aufgabe*, *Einstellung*, determining tendency, or nervous disposition. The most emphatic repudiation of act we find, again, among the existential psychologists. Titchener's principal objection to it is that it is unscientific, for it derives not from observation of mind out of use but from inference about mind as one finds it in use.[52] He suggests that the elusive experience which we erroneously call an act is in reality the processing, the going-on-ness, the duration of the content. "The way in which a process runs its course—that is its 'act,' that is what constitutes it sensing or feeling or thinking; the quality which is thus in passage—that is its 'content,' that is what constitutes it tone or pleasure." [53] But this suggestion is surely little more than a straitened effort to get the act over into sensation. On this view a bit of content that came and went in the twinkling of an eye would hardly have duration enough to build up the act-quality, or at best the act-quality would be about as hard to detect as James' darkness which is surprised by a flare from the gas-jet. But when some one asks you when Columbus discovered America you know perfectly well that the quick flash of 1492 was a memory. Neither is it easy to see how this view would account for the nice distinctions which one so readily makes in act-qualities. How can the same content be at one time a judgment and at another a memory if sheer processing of the content is itself the act-quality? Suppose I am presented with two tones and told to judge the interval formed by them. If, after making the judgment of a fifth, I am asked what I did, I most certainly am able to say that I made a judgment, not that I recognized the interval as the one in the opening

[52] 'Lectures on the Experimental Psychology of the Thought-Processes,' 1909, 60*f.*, 74.

[53] *Ibid.*, 61.

bar of Beethoven's 'Ninth.' Judgment and recognition are qualitatively distinct, as distinct, I should say, as a pair of tones and a pair of colors. No, the dimension of duration is not the same as the quality of an act. How do I know I made a judgment? Well, how do I know I heard two tones? Simply, I suppose, by keeping my psychological eyes open. Both were observations. "We find ourselves, and those about us," says Titchener, "deliberating, intending, resolving, planning, recalling, doubting." [54] But because this finding is an interpretation and not a description of palpables it is ruled out of psychology and labeled common sense. I protest. The finding appeals to me as a perfectly good bit of psychological observation and, to my way of thinking, is fully as significant as the observation of a pressury glow or a glassy sensation. The fact that an act-quality may not be adequately described in sensory terms is an important observation, not an observation which, because it is not an observation, should be ostracized from the correct company of observed palpables. But, it may be asked, if an act-quality is not sensory, what in the world is it? The same question has often been asked about feeling. To both questions I should return the same answer. Act quality and feeling are just what they are. The questions are no more confounding than would be the question, if sensation is not feeling, what is it? Why, sensation is sensory, of course. Look at it and see. Take a look also at feeling and act-quality.

The question before us relates to the possible nervous correlate of the psychological act-quality. In dealing with the quantitative aspects of various cognitive operations Spearman found that the amount or output of these operations varied from individual to individual, and within the same individual according as a given operation involved more or less *s*. He also noted that certain mental functions revealed a fairly constant degree of output. These facts he tried to account for on the hypothesis of a relation between the psychological tasks and a constant amount of nervous

[54] 'A Beginner's Psychology,' p. 6.

energy distributed variously for different functional groups of neurones. Now it should be noted that every one of the tasks which Spearman treated by his method of correlations was an act in Brentano's sense of the term. Is it not, then, a plausible assumption that *the release of nervous energy which determines the amount of output is reflected in consciousness by the unique act-quality?* In other words, the nervous energy has two psychological moments, the quantitative and qualitative. The quantitative moment is the amount of cognition, as revealed on some scale of measurement; the qualitative moment is the phenomenological character which cognition assumes: judging, deliberating, comparing, intending, reasoning, resolving, analyzing, recalling, and what not.

The acceptance of such a view, both on its factual and theoretical sides, implies, however, a point of view towards mind very like faculty psychology. We have already noted the resemblance between the scholasticism of Brentano's act psychology and the faculty psychology of mental powers and capacities. On the present view mind is conceived of as an intricate network of powers and capacities which are revealed in consciousness by act-qualities. We are hence brought back to the position outlined at the beginning of the paper, a position which we found was not looked upon with much favor. But I trust it has been possible to show that much of the adverse criticism directed against faculty psychology has been unwarranted.

Naturally any modern statement of faculty psychology will differ appreciably from its earliest formulations. Let us hope that psychology has not stood completely still for so many years. The most remarkable and important change has been wrought by Spearman. One of the criticisms urged against act psychology is that the logical reflection which produces the artifact of act can never get any further than the mere subtleties of classification. "I take the act-and-content psychology," says Titchener, "to be a psychology not of observation but of reflection. I note that it has led, in different hands, to very different classificatory systems. I think that Brentano found a difficulty in carrying it over

from the general to the particular." [55] This criticism must apply, I think, not to the materials which Brentano and his followers were attempting to classify, but only to the classification itself. The burden of proof against the existence of the materials, the act-qualities, rests with the Wundtians, and I question very much whether they are yet relieved of the burden. The classification, which, naturally enough, is not the same in different hands, any more than the classification of content is always the same in different systems of sensationalism, is the product of one of the tools of scientific method, and ought to be made uniform with the improvement of technique. That the technique of 1874 was not so good as that of today is hardly a reproach against Brentano. And that the logic of reflection can go beyond the preliminary steps of classification is amply shown by Spearman's achievement. There are two lines of attack by which he has sought to bring greater order into the field of cognition. The first consists of the application of his three fundamental qualitative laws of cognition and their corollaries to all intellectual operations. He argues that any act of cognition, when examined closely, turns out to be some variant of the eduction of relations or the eduction of correlates. The unique quality of the cognitive act still remains, but it is subsumed under the larger class of eductive acts. Much work needs to be done, it seems to me, along this line, but the point of departure which Spearman proposes is an extremely fruitful one for investigation. His second line of attack is quantitative. By the use of his tetrad equation he can determine the amount of overlap between any two abilities due to a common factor in the *s*'s. If the tetrad difference approximates zero there is no overlap; if it is significantly larger than zero there is overlap. Here is a tool which will go a long way towards nullifying any objection that mere classifying is all that one can do with acts. In the field of intellectual operations Spearman has developed two hypotheses which offer a possible means of discovering the interrelations of acts both with respect to their saturation by *g* and their degree of mutual dependence.

[55] 'Thought-Processes,' p. 53.

In 1907, in his famous essay 'The Energies of Men,' William James propounded two questions, the answers to which he felt would be of the very greatest significance to psychology. "(1) What are the limits of human faculty in various directions? (2) By what diversity of means, in the differing types of human beings, may the faculties be stimulated to their best results? Read in one way, these two questions sound both trivial and familiar: there is a sense in which we have all asked them ever since we were born. Yet *as a methodical programme of scientific inquiry*, I doubt whether they have ever been seriously taken up. If answered fully, almost the whole of mental science and of the science of conduct would find a place under them." [56]

The argument of the present paper has been that the concept of 'faculties,' in the sense of mental capacities distributed along quantitative continua and revealed phenomenologically as act-qualities, is of the utmost importance for any psychology which does not restrict itself to sensory existentialism; and that a methodical programme of scientific inquiry into the principles of human faculty is offered in the hypotheses and technique developed by Spearman for the study of cognition.

[MS. received August 3, 1928]

[56] 'Memories and Studies,' Chap. X., 236*f*.

A NEW CLASSIFICATION OF TONAL QUALITIES

BY CHRISTIAN A. RUCKMICK

University of Iowa

Diagrams and schema showing the classification and relationship of qualities within each sense department have at least a two-fold usefulness. Even though they may simplify too much our current scientific knowledge, they render a distinct service, especially to elementary students, in affording a visualization of the present status.[1] If the scientist has learned to regard such diagrams as not necessarily factual and final *per se*, they furnish furthermore a basis of discussion and a point of departure for theoretical considerations. The main source of danger lies in the presumption that figures, in either sense of the term, photographically or accurately reproduce undeniable facts. Schematic outlines and statistical data are both probably allied to portraiture in the sense that the scientist as well as the artist frankly admits that relations between things are interpreted as best he knows how.

The sensory group that has been most persistently schematized in terms of a visual diagram is the visual group itself. Here we find that the double pyramid, or octahedron, is quite generally adopted.[2] Because of the outstanding work of Henning the olfactory qualities have recently received schematic attention and from the same source increasing reference has been made to the taste tetrahedron. Titchener is responsible for the newest arrival in this field, the touch

[1] The author has put his convictions into practice in his recently published book, 'The Mental Life.' So far as he knows this is one of the few texts that have made extensive use of diagrams in connection with the various sensory qualities. At the present stage of our qualitative analysis no classificatory schemata have been proposed for the kinaesthetic and organic sense departments.

[2] The writer has recently consulted some twenty text-books and systematic treatises on this point. All but four authors definitely refer to the color pyramid while a few, like Warren, prefer to call it a spindle in view of its proportionally long axis. Two texts fail to mention any diagram, while one presents the visual scheme as a hexahedron, and another illustrates the qualities in terms of a circle.

pyramid.[3] But the figure requires further investigation and corroboration.

While several attempts have been made to organize the auditory qualities, apparently at the present writing there is no general acceptance of any one diagram. It is a remarkable situation in view of the fact that auditory phenomena have been under long and continuous scrutiny. Certainly no other group of sensations, save the visual sensations, has had so protracted a period of study. Although the visual qualities are thus fairly uniformly recognized and named, we do not have a similar state of affairs in regard to the auditory qualities. This is in part due to the practice of using descriptive terms for the auditory attributes which have not as yet become standardized.

The earliest attempt to depict the auditory qualities known to the writer was made by Drobisch in 1846.[4] In this scheme the chief principle was a spiral which gave the octaves the position of closest proximity in each return sweep of the curve. Ebbinghaus objected to this on two counts: (1) the relationship of the octave is excellent but the relation of the perfect fifth, the next best degree of fusion, is diagonally opposite; (2) tones most closely allied on the ascending spiral, *i.e.*, the diminished second, are most unlike in terms of the tonal character of fusion. He claimed, furthermore, that the octave could be reached on the spiral only through a gradual approach by means of intermediate and totally dissimilar tones. In 1897 Ebbinghaus himself proposed a unidimensional or serial arrangement which took cognizance of the pitch character of the tone, as its principal quality.[5] He added, however, that a spatial envisagement of the tonal series which could also take account of other qualitative likenesses, though much to be desired, was impossible.[6]

[3] Titchener, E. B., 'Models for the demonstration of sensory qualities,' *Amer. J. Psychol.*, 1920, 31, 213. See also Burnett, N. C., and Dallenbach, K. M., 'The experience of heat,' *Amer. J. Psychol.*, 1927, 38, 418–431; 'Heat intensity,' *ibid.*, 1928, 40, 484–494.

[4] Drobisch, M. W., 'Über die mathematische Bestimmung der musikalischen Intervalle,' 1846; also *Abh. d. kgl. sächs. Ges. d. Wiss. math.-phys.* Cl.B. II., 1855, 35.

[5] Ebbinghaus, H., 'Grundzüge der Psychologie,' Leipzig, 1902, 278.

[6] *Op. cit.* 280.

To meet the accumulating observations concerning the qualitative attribute of volume Titchener in 1909 proposed the tonal pencil.[7] In this diagram the attribute of size or diffusion, which is called 'volume' only because languages are full of spatial symbols, is represented by a second dimension making the scheme now for the first time a plane surface. The attribute of diffusion is relatively great in the bass notes, less marked and without much variation in the middle register, and then rapidly disappears in the high treble giving the pencil its point. The diagram has not gained wide currency in our texts, but it did contribute to further experimental work on the attribute of diffusion, or what has become known as 'volume.' Although in some quarters the attribute of volume is beginning to be seriously questioned we are now rapidly approaching the situation in which many psychologists have practically accepted the attribute of volume in the non-spatial significance.[8]

In the case of pitch we designate in all languages a quality which has no inherent spatial attributes through words that are steeped with spatial connotations. The meaning has here transcended the original import. We may be approaching the same situation with regard to the 'volume' of tonal

[7] Titchener, E. B., 'A Text-Book of Psychology,' 1909, 94. The study of 'volume,' which followed upon the presentation of this diagram is an indication of the theoretical utility of such diagrams. (*v.* Rich, G. J., 'A preliminary study of tonal volume,' J. EXPER. PSYCHOL., 1916, 1, 13–22; 'A Study of tonal attributes,' *Amer. J. Psychol.*, 1919, 30, 121–164; 'A note on "vocality",' *Amer. J. Psychol.*, 1921, 32, 446–447; Ogden, R. M., 'Hearing,' N. Y., 1924, 66–75; Ruckmick, C. A., 'Recent acoustic research,' PSYCHOL. BULL., 1924, 21, 606; Watt, H. J., 'The Psychology of Sound,' Cambridge, 1917, 27 *ff.*)

[8] The point here raised concerns usually some description distinctive between the sensation and the percept. In assigning attributive names to qualities in any sense department, there is always a suspicion of dealing with perceptual material. The most difficult problem in classificatory work is to remain absolutely naive. This of course has been the outstanding development in connection with the fate of the so-called 'vowel' quality. Were we not linguistically minded we would undoubtedly never think of classifying certain auditory qualities by their suggested vowel names. How heavy this latent *task* or *self-instruction* weighs upon trained laboratory observers only careful students of systematic psychology can understand. (*Cf.* Révész, G., 'Zur Grundlegung der Tonpsychologie,' 1913, footnote, p. 19.) In the last analysis this may be reduced to a level of a difficulty to be met rather than to the point of an irremovable obstruction in the path of research.

sensations. In our experimental work in this laboratory the reports of the *Os* designate 'volume' as a rule only metaphorically.

In the light of this more recent research by Rich and others, Ogden suggested a tonal manifold in which intensity, pitch, pitch-brightness, and volume each found its place.[9] The fundamental principle of this diagram rested upon a proposal by Watt that not only pitch but tonal masses must be considered.[10] The base line in the schemes of both Ogden and Watt indicate the relative thresholds for volume at successive pitches, the lowest in the series having a volumic spread inclusive of those assigned to each subsequent pitch. The chief criticism of these diagrams lies in the pronounced fact that for the first time in the history of spatial classifications we are including quantitative distinctions, *e.g.*, thresholds of intensity in otherwise qualitative diagrams. Even the color pyramid with its thousand different pure grays is considered essentially a qualitative schema. What the fate of pitch-brightness as distinct from pitch or volume will be no one knows, but it still continues to be generally discussed.

A substantial amount of consideration has been given, chiefly by Köhler, Meyer, Révész and Watt,[11] to a double usage of the terms pitch and 'tonal quality.' That there is a gradual change of pitch from one end to the other analogous to the change in the gray series from white to black among the visual qualities is quite generally admitted. But as these and other investigators indicate, a peculiarity in the tonal series exists which, with respect to the auditory sense department, is unique.

The double qualitative aspect of tonal sensations is further exemplified through numerous studies on absolute pitch

[9] Ogden, R. M., 'The tonal manifold,' PSYCHOL. REV., 1920, 27, 136–146. See also 'Hearing,' 69–75.

[10] Watt, H. J., *op. cit.*, 62–73.

[11] Köhler, W., 'Akustische Untersuchungen III,' *Zsch. f. Psychol.*, 1915, 72, 1–192. Meyer, M., 'On the attributes of the sensations,' PSYCHOL. REV., 1904, 11, 83–103; 'Vorschläge zur akustischen Terminologie,' *Zsch. f. Psychol.*, 1914, 68, 115–119; Review of Révész's 'Zur Grundlegung der Tonpsychologie,' PSYCHOL. BULL., 1914, 11, 349–351. Révész, G., *op. cit.*, 16 *ff.* Watt, H. J., *op. cit.*, 3–12.

consciousness and in colored hearing. In his elaborate study Baird discovered the difference between what he called 'note-response' and 'octave-response.'[12] He found that these "were usually separated by an appreciable interval; and the degree of subjective assurance attached to the octave-identification was usually very much less than that attached to the note-identification. The octave-error was common to the identifications of all nine of our observers; and it was present in the identifying of notes of every variety of clang-tint." He calls this one of the most striking features of his study and he terms it "the illusion of the octave." Révész arrives at the same result from his study which leads him to a similar double classification of tonal quality.[13] The only dissenting note in a long series of experiments in the field is that of Mull.[14] She says explicitly: "There is no objective evidence which necessitates the postulating of an attribute of quality, or tonality, which recurs in successive octaves and upon which all judgment of absolute pitch rests; nor do the introspective reports disclose the existence of any such attribute." In this study it must be noted however that the principal part of the investigation was confined to a single octave and that the possibility of octave confusion did therefore not really exist for these observers. Thorough though this experiment was, work ought to be continued throughout the musical range of pitch. If under those conditions introspections did not reveal the octave quality, then we certainly should have to take notice of such a result, history notwithstanding.

A somewhat similar situation occurs in the field of colored hearing. There are very few cases on record which show an absolute change of color for a single note from one octave to another. Most observers report when these colors are definitely seen that the notes an octave lower or higher differ

[12] Baird, J. W., 'Memory for absolute pitch,' 'Studies in Psychology,' (Titchener Commemorative Volume), Worcester, 1917, 69.

[13] *Op. cit.*, 16–19. Révész has further illustrated his dichotomy through the description of a pathological case in which the octave of the tone was correctly given but its pitch placement was defective in terms of a constant pseudotone of g♯.

[14] Mull, H. K., 'The acquisition of absolute pitch,' *Amer. J. Psychol.*, 1925, 36, 469–493.

mainly in tint or saturation or both; but a neighboring tone will have a radically different color ranging again in tint, saturation, or both tint and saturation in successive octaves. Recalling in that connection how definitely these colors are experienced and how fixed they are, even for a term of years as Langfeld has shown,[15] we ought to recognize the significance of a synæsthetic change in hue for the change of note as different from the change of tones an octave apart in tint and saturation.

From another source of investigation comes a corroborating argument. Brues would lead us to believe that the octave relationship in terms of simplicity of its vibration ratio must be excluded entirely from the field of fusion as a phenomenon of a different nature.[16] In a more recent study by Guernsey the distinction is made between fusion and consonance and the octave again is made to follow a separate sensory path.[17] This is somewhat in agreement with Stumpf's distinction between consonance and concordance. The upshot of all of this presumptive evidence is that the octave relationship is more intimately bound up with our perception of unison than with perceptions of consonance and in general supports the doctrine of a secondary tonal quality.

With this situation in mind and with a close perusal of other auditory investigations for the last few decades before him, the author tentatively proposes a figure that for lack of a better designation may be called a 'tonal bell.' [18] The pitch character of tonal sensations is represented by a continuous gradually ascending spiral line (painted red in the laboratory model). Adopting Drobisch's idea of the spiral, at each successive octave the ascending line brings these pitches into what may be called 'note relationship' or 'tonality'. These

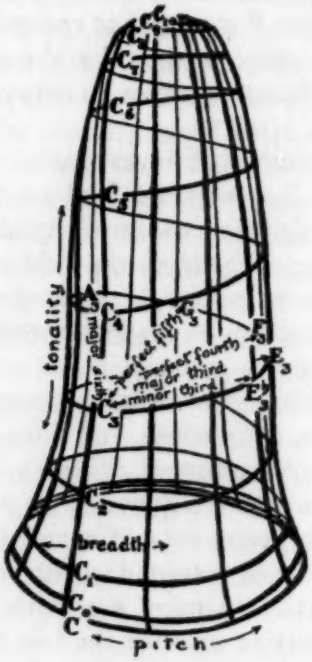
[15] Langfeld, H. S., 'A note on a case of chromaesthesia,' PSYCHOL. BULL., 1914, 11, 113–114.

[16] Brues, A. M., 'The fusion of non-musical intervals *Amer. J. Psychol.*, 1927, 38, 624–638.

[17] Guernsey, M., 'The rôle of consonance and dissonance in music,' *Amer. J. Psychol.*, 1928, 40, 173–204.

[18] The author has consulted a number of mathematicians and they are agreed that there is no technical term which fits this type of geometrical figure. If there is one the author would be glad to learn it.

note relationships are indicated by lines running generally in a vertical direction (painted blue in the laboratory model). The spiral starts somewhat below the lower auditory limit for tone (painted very dark red in the laboratory model) and



THE TONAL BELL

Reproduced from the author's 'The Mental Life' through the courtesy of Longmans Green & Co.

From Ruckmick, 'A New Classification of Tonal Qualities,' PSYCHOL. REV., 1929, 36, 178.

continues beyond the upper tonal limit (painted very light red in the laboratory model). The interval relationships, since they are phenomena resulting from a complex situation partly sensory, partly perceptual, and partly ideational, are indicated in the middle register by cross lines (painted yellow in the laboratory model). These are inserted simply for the convenience of the student but are not important in the classification of sensory qualities as such. Similar cross lines could be inserted, of course, in other octaves and for other intervals if desired.

The attribute of volume in one form or another originally indicated in the Titchener tonal pencil still has a large place in the literature. Just what its thresholds or differentiæ are must be further investigated.[19] By adopting the suggestions of Titchener the tonal bell follows in general the principle of Titchener's diagram. Volume, or what the present author prefers to call 'breadth,' consequently demands a broad base which gradually diminishes until the middle registers are reached, stays approximately uniform through this region, and then diminishes rather rapidly in the high registers. A cross-section through the tonal bell would therefore give an approximation to the tonal pencil. The length of the radius of each successive spiral roughly indicates, then, the breadth of that particular pitch attribute.

One other factor is represented in the tonal bell. It will be seen that the spirals rise with varying pitches. Putting it in another way the spirals are closer together in one region than in another. Assuming the vertical direction to be in general that of increase of pitch, the author has taken this means to show that according to our present data on pitch discrimination a tone may go the rounds of an octave without rising much in pitch in the lowest and highest regions of the scale. Investigators are in general agreed that pitch discriminations are most acute in the middle register. For the lowest and highest regions, of course, we have nothing at hand but estimates based on approximate geometrical continuations of the curve showing pitch discriminations. Boring has recently also raised the issue whether or not classifications of sensory qualities should be based solely

[19] In some recent investigations of Gundlach, reported at the last meeting of the Midwestern Psychological Association, doubt has been cast on the attribute of 'volume.' If further research should lead us to believe that volume like vocality is perceptual in character and not an attribute of auditory sensation, the figure would lose its 'bell' shape and become cylindrical without any definite significance to the length of its radius. This would leave its top and bottom surfaces uninhabited. But so at present are Henning's triangular faces without the recognition of triplex odors! This may nevertheless be a fault which points to the inadequacy of our present knowledge in the auditory field. At any rate it would do away with the criticism that in our diagram one set of attributes (pitch, and tonality) are represented by position on lines, while another set (volume) are shown in terms of distance from a central axis.

on the data obtained from judgments of discrimination.[20] But since this has been heretofore our general practice the author has made allowance for the large number of discriminable qualities on the basis of units calculated from the accompanying table. This is therefore responsible for the uneven rise of the spiral line.

Octave d.v.		*Pitch Discrimination* d.v.	units †
(C_7) 4096–8192	(C_8)	40*	100*
(C_6) 2048–4096	(C_7)	10	205
(C_5) 1024–2048	(C_6)	5	205
(C_4) 512–1024	(C_5)	2.5	205
(C_3) 256– 512	(C_4)	1.5	170
(C_2) 128– 256	(C_3)	1	128
(C_1) 64– 128	(C_2)	1.75	35
(C_0) 32– 64	(C_1)	4*	8*

* Estimates based upon continuations of the curve for determined discriminations.

† Calculated from the compass of each octave in terms of d.v. within that octave. Pitch discriminations in terms of d.v. multiplied by the respective units equals approximately the compass of each octave in terms of d.v. The above units were used to establish the amount of rise in the spiral above the base line at each successive octave level.

The author has put this diagram to the test of criticism in an attempt to clarify the present situation with respect to the tonal qualities. The diagram is essentially a qualitative figure. While still a considerable amount of work is required in order to stabilize and standardize our knowledge, experience dictates the advisability of presenting this diagram to a still larger group of critics for comment. It may serve the purpose not only of concretely presenting the issues which punctuate our present discussions but also of serving as a point of departure for future theoretical considerations.

[MS. received December 26, 1928]

[20] Boring, E. G., 'Auditory theory with special reference to intensity, volume, and localization,' *Amer. J. Psychol.*, 1926, 37, 157–188.

Psychological Review Publications

Original contributions and discussions intended for the Psychological Review should be addressed to

Professor Howard C. Warren, Editor Psychological Review,
Princeton University, Princeton, N. J.

Original contributions and discussions intended for the Journal of Experimental Psychology should be addressed to

Professor Madison Bentley, Editor Journal of Experimental Psychology,
Morrill Hall, Cornell University, Ithaca, N. Y.

Contributions intended for the Psychological Monographs should be addressed to

Professor Raymond Dodge, Editor Psychological Monographs,
Kent Hall, Yale University, New Haven, Conn.

Reviews of books and articles intended for the Psychological Bulletin, announcements and notes of current interest, and *books offered for review* should be sent to

Professor S. W. Fernberger, Editor Psychological Bulletin,
University of Pennsylvania, Philadelphia, Pa.

Titles and reprints intended for the Psychological Index should be sent to

Professor Walter S. Hunter, Editor Psychological Index,
Clark University, Worcester, Mass.

All business communications should be addressed to
Psychological Review Company, Princeton, N. J.

The Psychological Review
is indexed in the
International Index to Periodicals
to be found in most public and
college libraries

DIRECTORY OF
American Psychological Periodicals

American Journal of Psychology—Ithaca, N. Y.; Cornell University. Subscription $6.50. 624 pages ann. Ed. by M. F. Washburn, K. M. Dallenbach, Madison Bentley and E. G. Boring. Quarterly. General and experimental psychology. Founded 1887.

The Pedagogical Seminary and Journal of Genetic Psychology—Worcester, Mass.; Clark University Press. Subscription $7.00. 700 pages ann. Ed. by Carl Murchison. Quarterly. Child behavior, differential and genetic psychology. Founded 1891.

Psychological Review—Princeton, N. J.; Psychological Review Company. Subscription $5.50. 500 pages annually. Bi-monthly. General. Founded 1894. Edited by Howard C. Warren.

Psychological Bulletin—Princeton, N. J.; Psychological Review Company. Subscription $6.00. 720 pages annually. Psychological literature. Monthly. Founded 1904. Edited by Samuel W. Fernberger.

Psychological Monographs—Princeton, N. J.; Psychological Review Company. Subscription $6.00 per vol. 500 pp. Founded 1895. Ed. by Shepherd I. Franz. Published without fixed dates, each issue one or more researches.

Psychological Index—Princeton, N. J.; Psychological Review Company. Subscription $4.00. 300–400 pp. Founded 1895. Edited by W. S. Hunter. An annual bibliography of psychological literature.

Journal of Philosophy—New York; 515 W. 116th Street. Subscription $4. 728 pages per volume. Founded 1904. Bi-weekly. Edited by F. J. E. Woodbridge, Wendell T. Bush and H. W. Schneider.

Archives of Psychology—Columbia University P. O., New York City. Subscription $6. 500 pp. per vol. Founded 1906. Ed. by R. S. Woodworth. Published without fixed dates, each number a single experimental study.

Journal of Abnormal Psychology and Social Psychology—Albany, N. Y. Subscription $5. Boyd Printing Co. Ed. by Morton Prince. In cooperation with Henry T. Moore. Quarterly. 432 pages ann. Founded 1906. Abnormal and social.

Psychological Clinic—Philadelphia; Psychological Clinic Press. Subscription $3.00. 288 pages. Ed. by Lightner Witmer. Founded 1907. Without fixed dates (9 numbers). Orthogenics, psychology, hygiene.

Training School Bulletin—Vineland, N. J.; The Training School. Subscription $1. 160 pages ann. Ed. by E. R. Johnstone. Founded 1904. Monthly (10 numbers). Psychology and training of defectives.

Comparative Psychology Monographs—Baltimore; Williams & Wilkins Co. Subscription $5. 500 pages per volume. Edited by W. S. Hunter. Published without fixed dates, each number a single research.

Psychoanalytic Review—Washington, D. C.; 3617 10th St., N. W. Subscription $6. 500 pages annually. Psychoanalysis. Quarterly. Founded 1913. Ed. by W. A. White and S. E. Jelliffe.

Journal of Experimental Psychology—Princeton, N. J. Psychological Review Company. 500 pages annually. Experimental. Subscription $6.00. Founded 1916. Bi-monthly. Ed. by Madison Bentley.

Journal of Applied Psychology—Baltimore, Md.; Williams & Wilkins Co. Subscription $4. 400 pages annually. Founded 1917. Quarterly. Edited by James P. Porter and William F. Book.

Journal of Comparative Psychology—Baltimore; Williams & Wilkins Company. Subscription $5. 500 pages annually. Founded 1921. Bi-monthly. Edited by Knight Dunlap and Robert M. Yerkes.

Genetic Psychology Monographs—Worcester, Mass.; Clark University Press. Subscription $7.00 per volume. Two volumes per year, 600 pages each. Ed. by Carl Murchison. Monthly. Each number one complete research. Child behavior, differential and genetic psychology. Founded 1925.

Psychological Abstracts—Eno Hall, Princeton, N. J. Edited by W. S. Hunter. Subscription $6.00. Monthly. 700 pages annually. Founded 1927.

Journal of General Psychology—Worcester, Mass. Clark University Press. Subscription $7.00. 600–700 pages annually. Edited by Carl Murchison. Quarterly. Experimental, theoretical, clinical, and historical psychology. Founded 1927.

PR.